NATURAL SCIENCE:

A Monthly Review of Scientific Progress.

No. 32. Vol. V. OCTOBER, 1894.

NOTES AND COMMENTS.

Protozoa from pre-Cambrian Rocks.

ONE of the most interesting contributions to geological and biological science of late is Monsieur L. Cayeux's report on the supposed Radiolaria and Foraminifera from the pre-Cambrian rocks of Brittany. About two years ago we alluded to Dr. Charles Barrois' discovery of these remarkable fossils, and announced the preparation of a detailed report by M. Cayeux. The results have now appeared in the *Bull. Soc. Géol. France* (ser. 3, vol. xxii., pp. 197–228, pl. xi.), and in the *Comptes Rendus* (vol. cxviii., pp. 1433–1435); and there is a valuable, cautiously-worded notice by Dr. George J. Hinde in the last number of the *Geological Magazine* (Dec. 4, vol. i., pp. 417–419). It appears that the supposed organisms occur in thin layers of flinty rock interstratified with the pre-Cambrian schists, and they are irregularly distributed, sometimes being met with singly, sometimes grouped together in clusters. They are much more minute than any known Radiolaria, the average size of those of the Palæozoic rocks being about seventeen times as great. The majority are simply spherical in form, some are ellipsoidal, and a few are inflated or bell-shaped. Some specimens exhibit radial spines, while others display an inner concentric shell connected by rays with the outer test; and nearly all of the best preserved examples have a lattice-like structure. M. Cayeaux recognises forty-five different forms of these fossils, and assigns the majority of them to known genera, while the whole may be comprised in the subordinal divisions of Radiolaria established by Haeckel.

Many of the minute bodies found with the Radiolaria, and originally supposed to be of the same nature, are now regarded by M. Cayeux as Foraminifera; and these also, it is interesting to note, differ from later forms in being much more minute. Some of the shells are

simple, others compound, and the latter consist of from two to seven spherical or ovoid chambers of different sizes aggregated together. The walls are very finely perforate, and some of the chambers are provided with one or more short blunted spines.

Much discussion will doubtless arise in reference to these new problematical bodies, and, so far as we can judge, there is some hope that they will stand the test of criticism better than the ill-fated *Eozoon*. Much confirmation, however, is yet required, and we look forward with interest to the discovery of similar forms in the pre-Cambrian rocks of our own island.

Primæval Life.

Theories as to what the nature of the earliest life must have been are still flourishing, and in the August number of the *Journal of Geology* Professor W. K. Brooks has a fascinating, interesting, and exceedingly suggestive, if not absolutely convincing, article on the subject. The paper bears so directly on the discussions at the recent meeting of the British Association as to the distribution of marine animals, that we give here an outline of its main points, which we hope will send our readers to the original. In many respects it reminds us of the views of the late Professor Moseley.

The variety, distinctness, and advanced structure of the animals found in the very lowest fossiliferous rocks have long puzzled geologists, and have seemed, on the theory of evolution, to necessitate an enormous backward extension of life before the beginning of the Cambrian period, although no tangible proof of the existence of such life is forthcoming in rocks where it ought to be found. It is this difficulty that Professor Brooks attempts to explain.

In a few vivid pages we are shown how the ocean is almost destitute of plant-life, how its flower gardens are stocked only with animals, and how its very herbs and lichens are corallines and sponges; we learn how the vast animal armies of the sea attack and devour other animal armies, but have never a plant for their forage; and thus, by descending steps, we are brought to the conclusion that "the basis of all the life in the modern ocean is found in the micro-organisms of the surface," lowly animals and plants so abundant and prolific that they meet all demands. These minute pelagic creatures must have been the first to exist, and where they first appeared there they have ever since remained, undisturbed by varying environment or the stress of competition, and therefore retaining their primæval simplicity.

The early pelagic fauna gave rise to a few simple types, such as we now know only in the larval forms of higher animals. But further development never took place here.

On the contrary, we recognise that all highly organised marine animals are products of the bottom or of the shore, or, as in the case

of the whales, of the land; even the pelagic animals of higher grade have come back again to the surface from ancestral homes elsewhere.

There was, however, a time when the bottom and shores of the ocean were untenanted; when, perhaps, physical conditions made their population impossible. Life at that time occurred only in simple forms and near the surface of the central sea. At last the bottom was discovered and colonised. The earliest settlements were not in shallow water, where food supply was scanty and mixed with sediment, nor in the great depths as yet unfavourable to life; but it is inherently probable, as well as confirmed by palæontological evidence, that the bottom life first found a footing in the deep water around elevated areas. Colony after colony may, it is true, have been "swept away by geological change like a cloud before the wind"; but when once the outlines of our modern continents were blocked in, then "the first fauna which became established in the deep zone around" them "may have persisted and given rise to the modern animals."

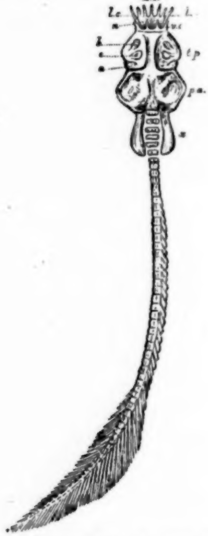
The sudden and enormous change of conditions accompanying this change of residence soon made itself felt on the creatures of the deep. The results were increase of asexual multiplication and the establishment of colonies, crowding followed by competition and more exacting selection, the acquisition of hard parts, and increase of size. Progressive evolution along these lines would have required no lengthy period to develop, out of those pelagic and soft-bodied ancestors that geologists will never discover, a fauna in all respects similar to that which geologists have already discovered in the Lower Cambrian rocks.

Thus those æons which the geologist has loved to imagine as his own, which the evolutionist has demanded, but which Lord Kelvin has denied and Lord Salisbury ridiculed,—they for Professor Brooks are but the baseless fabric of a dream, unproved, unreal, and unneeded.

The Primæval Lampreys Again.

Even of the earliest animals which possessed well-developed hard parts, we still know very little; and it is strange how peculiarly accidental are the few important discoveries that have been made. The little Devonian lamprey *Palæospondylus*, for instance, is known to us merely through the circumstance that the Caithness quarrymen happened to alight upon a little patch of good flagstone at a remote spot named Achanarras. It has been found hitherto only in that one quarry and is restricted to one narrow layer of rock. *Palæospondylus*, however, revolutionises our old ideas of the Class Marsipobranchii, and we have already published communications in Natural Science pointing out its significance. Quite lately still more information has been obtained on the subject, and Dr. Traquair's latest restoration of

the skeleton, taken from the newly-issued part of the *Proc. Roy. Phys. Soc. Edinburgh* (vol. xii., pp. 312–320, pl. ix.), is reproduced below for comparison with those previously attempted. It now appears that the animal had a median opening or ring, surrounded with feelers, at the front of the head; this being presumably comparable with the nose of a modern lamprey. It is also evident that the fin-supports at the end of the tail are longer than previously suspected; and those at



Palæospondylus gunni; new restoration by Dr. R. H. Traquair, somewhat enlarged. *d.c.*, dorsal cirri; *l.c.*, lateral cirri; *n.*, nasal opening; *p.a.*, periotic region of skull; *t.p.*, anterior part of skull, with indents marked *a.*, *b.*, and *c.*; *v.c.*, ventral cirri; *x.*, problematical (? branchial) plates.

least of the dorsal aspect are proved to bifurcate towards the top, exactly as in a lamprey. That *Palæospondylus* is a primæval Marsipobranch is thus suggested still more forcibly than ever, and the next important step is to discover the links between this little organism and some of its yet more remarkable contemporaries, *Pteraspis* and *Cephalaspis.*

The Armoured Fore-runners of the Chordata.

Remarkable additions to our knowledge of the last-named animals, now commonly assigned to a subclass termed Ostracodermi or Ostracophori, have lately been made by Dr. J. V. Rohon and Dr. Friedrich Schmidt in the publications of the St. Petersburg Imperial Academy of Sciences. The descriptions, both of the form of the shields of *Tremataspis*, *Auchenaspis* (*Thyestes*), and fragments of allied genera, are supplemented by a detailed, well-illustrated account of

their minute structure. Moreover, the explorations of Herr Simonson in the Upper Silurian Limestone of the Island of Oesel, in the Baltic, have recently furnished most of the principal European museums with good specimens. The fossils are thus within reach of most workers. The greatest sensation of late, in connection with this subject, however, is the announcement by Mr. William Patten, in the *Anatomischer Anzeiger*, that certain parts of the shield of the existing king-crab, *Limulus*, are almost identical in intimate structure with a certain layer in the shield of the Devonian *Pteraspis*. We have not yet had the opportunity of verifying the observation; but if the facts can be established Palæontology will, after all, furnish some support to the idea that there is a close connection between the primitive Chordata and the so-called Arachnids of the *Limulus* tribe. We prefer, however, at present to leave the question of the ancestry of the Chordata to the Embryologists, and we cannot do better than allude next to recent researches in this direction.

The Ancestry of the Chordata.

The researches of the past ten years have shown a marked tendency towards a closer and closer *rapprochement* between those three groups of enterocœlous animals, the Echinoderma, the Enteropneusta, and the Chordata. The idea of an affinity between the two first groups dates from the time when Agassiz in America and Metschnikoff in Europe discovered that *Tornaria*, which had been regarded by Johannes Müller as a starfish larva, was in reality the larva of the worm-like *Balanoglossus*. Metschnikoff even went so far as to homologise the proboscis of the adult *Balanoglossus* with an ambulacral tentacle, and the proboscis-vesicle he regarded not simply as a rudimentary, but as an actually vestigial representative of the water-vascular system of the Echinoderm. It is chiefly to Mr. W. Bateson, on the other hand, that we owe the establishment of the Chordate affinities of the same remarkable animal; while the whole progress of Echinoderm research, in the able hands of Ludwig, Semon, Bury, and others, has revealed a continually-increasing series of fundamental resemblances between the Echinoderm and the Chordate types. The idea of a real affinity between the three groups has taken deep root, especially in England, and the somewhat ill-founded opposition of Spengel to the view of the Chordate relations of *Balanoglossus* has effected little beyond the strengthening of the position of those who dissent from his conclusions.

Mr. Garstang expounded a theory of the inter-relationships of these groups, which he takes to present a simple and consistent explanation of the structure and development of *Balanoglossus* and the lower Chordata.

Recent research has shown that the larvæ of Echinoderms are all derivable from one common type, which can be imagined as a

simple *Auricularia* larva, possessing a terminal blastopore, a continuous circumoral (longitudinal) ciliated ridge, with a subjacent central nervous system, and an adoral ciliated band, which extends round the anterior and lateral margins of the mouth, and is drawn inwards and backwards in the mid-ventral line to form a ventral œsophageal ciliated loop.

The *Tornaria*-larva of *Balanoglossus* is equally derivable from the *Auricularia*-type; and now Mr. Garstang suggests that the true Chordata also trace their origin from the same primitive pelagic form, thus following out in a definite manner the interesting views of Brooks in this direction. (See Natural Science, vol. iii., p. 222.)

The basis of his theory is the homology which he proposes between the medullary folds of vertebrate embryos and the paired circumoral longitudinal ridge of *Auricularia*, the homology being founded on the identity of the relations of these two sets of structures to the mouth, the apical pole, the blastopore, and the central nervous system. In the Chordata the ciliated medullary ridges eventually fuse to form the medullary or neural canal, thus enclosing the sense-organs of the apical pole (optic pits) and the blastopore.

In the Echinoderma, as was shown by the beautiful researches of Semon, the central nervous system becomes segregated at an early period from the ciliated ridges, and migrates ventrally to form the circumœsophageal nerve-ring, while the ridges eventually atrophy and the blastopore persists as the anus.

An especial importance naturally attaches to the relations of the nervous system and ciliated bands of the developing *Balanoglossus*. The diagrams exhibited at the reading of the paper, and the new point of view from which the author regarded the course of the nerve-tracts in *Balanoglossus*, undoubtedly revealed some most significant, if not decisive, evidence in favour of his theory. Unlike his predecessors, Mr. Garstang holds that the dorsal nerve-cord in the trunk of *Balanoglossus* is not the morphological continuation of the collar-cord, but is —along with the ventral cord—a nerve-tract that is entirely confined to the group Enteropneusta, developed *pari passu* with the posterior elongation of the body. He holds, on the other hand, that the collar-cord, though ontogenetically median and unpaired, has been phylogenetically derived by the fusion of two lateral cords, and he finds support for this view in its divarication posteriorly to form the nerve-ring which runs along the posterior rim of the collar. The collar-ring is upon this view homologous with the periblastoporal portion of the nerve-plate in vertebrate embryos, and with the posterior transverse loop of the central nervous system of *Auricularia*.

As to the medullary folds of the collar which give rise to the neural canal, it was pointed out that, according to the researches of Morgan on *Tornaria*, the medullary folds do not cease at the posterior limit of the collar-cord, but bend round the sides of the body and *persist through life as the posterior rim of the collar*, the area of separation

in the mid-dorsal line being reduced and obliterated by the union of the longitudinal portions of the folds to form the neural canal. Attention was also drawn to the facts that the medullary folds of *Tornaria* not only arise in close relation with the degenerating posterior portion of the ciliated band, but take a course which is identical with it; and that the region of the neural tube is just that area where the convoluted ciliated bands approximate most closely in the dorsal region of the body.

The position of the gill-slits in *Balanoglossus* behind, and in Chordata in front of, the neural loop was attributed to that peculiar change of position which the primitively cæcal gill-pouches of *Balanoglossus* undergo during the period of metamorphosis, as observed and described both by Agassiz and Morgan.

Pending the appearance of the author's complete account, we need say no more here, except, perhaps, that Mr. Garstang finds in the peculiar adoral ciliated band of *Auricularia* the exact precursor of the combined peripharyngeal bands and the marginal bands of the endostyle of the Tunicata and *Amphioxus*.

The Ancestry of Modern Mammalia.

In the *American Journal of Science*, Professor O. C. Marsh is continuing his valuable brief notes on the remains of Tertiary mammals from the western regions of North America, while Professors Cope, Scott, and Osborn, and Mr. Earle, are engaged upon more elaborate memoirs. Progress is thus rapid in the northern half of the New World, and Mr. Lydekker has spent the autumn in further work upon the remarkable extinct Tertiary mammalia, chiefly sloths and armadillos, in its southern half. Of greatest interest, however, is the announcement that the general public in North America will soon have the opportunity of seeing these extinct quadrupeds displayed in an easily comprehensible manner, so that they can judge of the beasts for themselves. Except at Princeton, the collections of mammalian remains have hitherto been stowed away almost like goods in a store; but the American Museum of Natural History has now for some time secured the services of several good collectors, including Mr. J. B. Hatcher, and numerous entire skeletons of the most typical Tertiary animals are, as a result, at present being mounted for the exhibition rooms. Professor Osborn, of Columbia College, is superintending the work, and as the mounting proceeds, drawings of the actual specimens will be published.

European Pleistocene Mammalia.

In Europe similar studies are also progressing favourably, and an enterprising dealer, we understand, has been making extensive excava-

tions in the Pliocene deposits of Pikermi and the Island of Samos. Dr. Forsyth Major's memoir on the Pliocene Mammalia of Samos will soon appear in the *Palæontographica,* and his collection from Olivola, in the North of Italy, is now being extricated from matrix at the British Museum.

Much greater progress, however, has been made of late in the study of Pleistocene Mammalia. M. Edouard Harlé has been especially energetic in the South of France, and has found much of interest in the caverns of Haute Garonne (*Bull. Soc. Géol. France,* ser. 3, vol. xxii., pp. 234–241, and *L'Anthropologie,* vol. v., pp. 402–406). His most interesting discoveries consist in further evidence of the occurrence of the striped hyæna, and of the elk in Southern France. Professor Lartet, thirty years ago, was of opinion that the striped hyæna migrated northwards from Africa beyond the Pyrenees, at the time when Europe and Africa were connected by barriers; but considering that teeth almost identical with those of the striped hyæna have also been found in the Pliocene both of France and England, M. Harlé now regards it as much more likely that the animal met with in the caves was a direct descendant of its predecessor in the European region. The confirmation of the discovery of the elk in south-west France is interesting, because the evidence has not previously been very satisfactorily described. Mr. Harlé now gives a figure and description of an upper molar tooth, which he considers to be unmistakable.

Pleistocene Mammalia from the Thames.

In England, one of the most interesting discoveries of Pleistocene bones lately made is that described by Messrs. J. R. Leeson and G. B. Laffan in the last number of the *Quart. Journ. Geol. Soc.* These authors have found a bed of loam deep in the Thames gravels at Twickenham, and here, or immediately above it, there occur the remains of the reindeer, bison, and oxen. The only known fragment of the Saiga Antelope found in Britain was also obtained from the same neighbourhood. One reindeer antler and a skull of bison are especially fine, and these specimens have been presented by Dr. Leeson to the British Museum. The most interesting feature of the discovery, however, is the possible occurrence of the Celtic shorthorn in association with the above-mentioned remains. We are by no means convinced that the authors have made out their case for this modification of current views; for all discoveries hitherto have tended to show that the shorthorn was introduced into Britain by the Neolithic people who invaded the country after it became an island. Nevertheless, the mode of occurrence of the bones at Twickenham is suggestive of their all belonging to one period, and there seems to be no doubt that some specimens do belong to the small ox in question.

Acclimatised Rats and Cats.

A new interesting case of the gradual adaptation of animals to their surroundings is quoted in a recent number of *Public Opinion* from the *Pittsburg Dispatch*. It appears that in the cold-storage warehouses in Pittsburg there were originally no rats or mice. The temperature in the cold rooms was too low. The keepers soon found, however, that the rat is an animal of remarkable adaptability. After some of these houses had been in operation for a few months, the attendants found that rats were at work in the rooms where the temperature was constantly kept below the freezing point. They were found to be clothed in wonderfully long and thick fur, even their tapering snake-like tails being covered by a thick growth of hair. Rats whose coats have adapted themselves to the conditions under which they live have domesticated in all the storage warehouses in Pittsburg. The prevalence of rats in these places led to the introduction of cats. Now, it is well-known that pussy is a lover of warmth and comfort. Cats, too, have a great adaptability to conditions. When cats were turned loose in the cold rooms they pined and died because of the excessive cold. One cat was finally introduced into the rooms of the Pennsylvania Storage Company which was able to withstand the low temperature. She was a cat of unusually thick fur, and she thrived and grew fat in quarters where the temperature was below 30 deg. By careful nursing a brood of seven kittens was developed in the warehouse into sturdy, thick-furred cats that love an Icelandic clime. They have been distributed among the other cold storage houses of Pittsburg, and have created a peculiar breed of cats, adapted to the conditions under which they must exist to find their prey. These cats are short-tailed, chubby pussies, with hair as thick and full of under fur as the wild cats of the Canadian woods. One of the remarkable things about them is the development of their "feelers." These long stiff hairs that protrude from a cat's nose and eyebrows are, in the ordinary domestic feline, about three inches long. In the cats cultivated in the cold warehouses the feelers grow to a length of five and six inches. This is probably because the light is dim in these places, and all movements must be the result of the feeling sense. The storage people say that if one of these furry cats is taken into the open air, particularly during the hot season, it will die in a few honrs. It cannot endure a high temperature, and an introduction to a stove would send it into fits.

Living Specimens of Protopterus.

Those zoologists who attended the conversazione given by the local committee to the members of the British Association in the University Museum, had the opportunity of seeing a zoological exhibit of very unusual interest. Dr. H. O. Forbes, the Director of Museums, Liverpool, exhibited on behalf of the Derby Museum living specimens

of the African dipnoid *Protopterus annectens*, encased in the blocks of mud in which, during the dry season, these singular fishes bury themselves. They had been obtained for the museum from one of the West African rivers, at a distance of nearly 500 miles from the sea, through the exertions of Mr. Ridyard, of Liverpool. The fish had been in their mud-cocoons for over five months, having been kept in England for more than two months. Dr. Forbes prepared in the large lecture room of the University Museum glass jars of tepid water and into these the cocoons were placed in the presence of visitors. The hard rock-like clay became softened, and one of the fish soon came out in a lively condition and swam about freely. Mr. Forbes stated that while some specimens in his possession had emerged in five minutes, others had taken at least an hour. Of course, living specimens have been brought to Europe in this condition before now —there are at present a number of specimens in the reptile-house of the Zoological Gardens—but we fancy that very few of the members of the Association have had before, or are likely to have again, so interesting a practical demonstration of the habits of these animals.

The Plague Bacillus.

An authoritative account of the bacillus of the Oriental plague is at length to hand from the pen of its discoverer, Professor S. Kitasato of Tokio. We learn from the *Lancet* of August 25 that it is a capsuled bacillus, scarcely, if at all, motile, and exhibiting a tendency to bipolar staining. It is present abundantly in the blood and internal organs, and especially in the spleen and bubonic swellings of victims of the disease. It can be grown readily on suitable cultural media at the temperature of the human body, and rodents inoculated with pure cultures die in from two to five days, with symptoms and pathological changes resembling those of the natural disease in man. Rodents seem, indeed, to be highly susceptible, rats and mice having died in large numbers in Hong Kong during the epidemic, as they are related to have done during the historic visitations of the disease in this country.

All these facts are in complete harmony with what might have been anticipated, and the excellent work done by Professor Kitasato in Koch's laboratory in Berlin should leave little room for doubt that his discovery is a genuine and an important one. As was natural, however, the supremacy of this particular organism has not been allowed to pass unchallenged, and other claimants for the doubtful honour of being the real cause of the plague have been put forward by Dr. Yersin and others. Final judgment as to the respective merits of the rival microbes must be postponed till fuller details are forthcoming. No doubt securely bottled samples will in time arrive in this country, where it is to be trusted they will be kept under lock and key. Professor Kitasato has, however, already shown that the powers

of resistance of his plague bacillus are not very high, as it seems to form no spores. Four days desiccation kills it, as does exposure to sunlight for three or four hours in the dried condition. The temperature of boiling water is fatal in a few minutes, and a one per cent. solution of carbolic acid kills in an hour. So that we shall know in a measure how to deal with this bacillus in case it ever does break loose among us. It seems, moreover, that the plague is essentially a filth disease, and would stand little chance in presence of decent modern sanitation.

Small-pox.

The recent outbreak of small-pox in London, though the prompt measures taken seem to have been effectual in confining it within moderate limits, may serve to remind us how difficult a thing it is to fight against an enemy whose exact nature we do not know. Empirically we have learned much as to the prophylaxis and treatment of the disease, and now that experiment has demonstrated that vaccinia is merely small-pox modified by transmission through the cow, we have a rational scientific basis for the practice of vaccination. Yet, though small-pox is always with us, and in spite of the fact that experimental inoculation of mankind with its attenuated virus is rendered compulsory by law, the actual nature of the virus remains still under dispute. It is rather the fashion nowadays, when no bacterium can be demonstrated in a given specific disease, to invoke the Protozoön. That Protozoa may cause disease we know—witness malaria—but the utmost caution is required in interpreting the appearances to be seen in epithelial cells stained with fancy dyes, and the protozoan origin of small-pox, as of cancer, rests at present on a very slender foundation. And indeed there is now some evidence that the virus of small-pox may after all be a spore-bearing bacillus which has hitherto eluded observation by its habit of turning into an unstainable spore at an earlier period than that at which anyone had thought of looking for it. Both Dr. Klein and subsequently Dr. S. Monckton Copeman have independently discovered that such a spore-bearing bacillus is to be demonstrated in the lesions of small-pox and vaccinia, if search be made at a sufficiently early stage of the disease, and though cultivations have uniformly failed, there seems at least to be a possibility that this organism may prove to be the true cause of small-pox.

In a supplement dated 25th May last the *Leeward Islands Gazette* reports a paper read by Mr. C. A. Barber at the inaugural meeting of the St. Kitts branch of the Leeward Islands Agricultural Society. The paper deals chiefly with a disease affecting the sugar-cane, known as the Rind-fungus (*Trichosphæria Sacchari*), and described as the most

dangerous and widespread enemy of the cane cultivation of the present day, occurring in greater or less abundance probably everywhere where the plant is grown. Associated with the fungus is a moth-borer caterpillar; in fact, the insect seems to be the first cause of the disease, as the fungus cannot penetrate the sound plant. The young grubs immediately after hatching upon the leaf-blades make their way down to the protecting leaf-sheath, where they burrow in the soft inner tissues till strong enough to penetrate the cane. The burrows of the moth-borer give entrance to the fungus, and are noticeable as centres of infection. The fungus then rots the cane Various suggestions are made for the destruction of the moth-borer, and the selection of canes capable of resisting the attacks of the fungus.

Let the humble student of British botany rejoice. A month or two ago we referred to the Brambles as the happy hunting grounds of a little band of clerics. But the Hawkweeds seem even more remunerative, for in the *Journal of Botany* for August, Mr. F. J. Hanbury, who is tardily and expensively monographing the genus, diagnoses no less than seven new species and two-and-twenty new varieties. As their names indicate, two, *Hieracium surrejanum* and *H. cantianum*, grow close at home, in fact, within fifty miles of London. We do not dread their extermination, in spite of such proximity, except by accident, for like the Brambles and Roses these species are beyond the ken of the ordinary collector, and to quote Mr. Hanbury himself in his mild censure of his Scandinavian colleagues, we "feel it necessary to express a grave doubt as to the practical utility of of such subdivision," and our fear that the study of these genera will be rendered an impossibility, except to the few specialists who may devote their entire lives to their elucidation.

A note on the germination of seeds in sawdust in the *Botanical Gazette* for August will be of interest to teachers of Botany. When all goes well there is no better way of getting clean straight roots for class or experimental purposes; but, unfortunately, the sawdust has frequently a detrimental effect, and roots of plants germinated therein are seen to be in a very unhealthy condition. This, Mr. Stone points out, is due to the presence of tannin, and if only care be taken to select dust free from this, results will be quite satisfactory. Roots grown in sawdust containing tannin show a reddish colouration, are crooked, and much reduced in size, and this abnormal appearance may be brought about by saturating any sawdust which has been proved suitable for normal growth with a one per cent. solution of tannin. Oak and chestnut dust are, for this reason, to be avoided, while that obtained from Conifers has no prejudicial effect. The writer adds: "We have never experienced difficulty with any sawdust which failed to give the tannin reaction, and during the past year we have used the same sawdust continually without changing."

In the same journal is a *résumé*, by J. C. Bay, of observations and experiments in the formation of crystals of ice on plants. The splitting of wood and the appearance of ice-crystals in the fissures are phenomena well known to tree planters. As a result of the cold, the tissues of the whole plant contract, and consequently their turgescence is much diminished, as well as the permeability of the cell-walls to water. The contents of the peripheral ends of the medullary rays freeze, expand, and are pressed forward, causing a split in the stem at the point of least resistance. The ice forms a layer covering the whole surface of the wound. The internal pressures continue to supply water, by which the ice-sheet is continually augmented.

An ancient oak formed the subject of an interesting exhibit at one of the last meetings of the Linnean Society, when Mr. Carruthers showed some photographs of an old tree which still grows at Conthorpe, in Yorkshire. Prints prepared some hundred years ago show no departure from the normal habit of a fine oak, but recent photos illustrate a remarkable change in mode of growth. The successive ramifications of the thick lower branches no longer spread in all directions, but the latter produce a number of erect shoots giving the appearance of young trees growing from the horizontal main branch, which may be compared with a rootstock sending up vertical shoots, as happens in the Solomon's seal, species of *Polygonum*, and many creeping plants.

German botanists are still busily working at the tropical African flora, to which the recently-issued number of Engler's *Botanisches Jahrbuch* (vol. xix., parts 2 and 3) supplies a further contribution. This includes a large number of new Labiatæ, Orchids, Thymeleaceæ, and others. In the *Journal of Botany* for September, South African botany is represented by a paper by R. Schlechter, who describes some new species of Asclepiadaceæ from Natal and elsewhere.

Some time ago we gave an account of Magnin's work on the flora of the Jura Lakes. During the summer of 1893, under the auspices of the Michigan Fish Commission, the flora and fauna of Lake St. Clair have been studied in a similar manner. Mr. A. J. Pieters, the botanist of the party, states that the flora was found arranged in zones, limited by the depth of the water, each with certain characteristic plants, but Magnin's Nupharetum did not exist.

News has arrived from Mr. G. F. Scott Elliot, in a letter dated Ruwenzori, the 24th of May, in which he states that he has been on the mountain for nearly two months, collecting and exploring under

somewhat unfavourable conditions. In spite of fever, which has kept him to his bed for half the time, Mr. Elliot has been able to get to 11,000 feet, where he found a violet and a tree heath. A deciduous forest extends from 7,600 to nearly 9,000 feet, above which are bamboos. The forest is damp to a degree, a cloud usually hanging over it the whole day, making the processes of collecting and drying rather difficult.

THE English Dialect Society is anxious to include the supplement to the *Dictionary of English Plant Names* among its publications for 1894. Those who have any additional names or notes are invited to send them as soon as possible to Mr. James Britten, 18 West Square, Southwark, London, S.E.

A COLLECTION of twenty-three human skulls made by Dr. Machon in the caves and old cemeteries of Patagonia, is the subject of a detailed illustrated description by Dr. R. Verneau in the last number of *L'Anthropologie* (vol. v., pp. 420–450). Seven specimens have been presented to the Paris Museum of Natural History. Dr. Verneau confirms the conclusion already arrived at by Dr. F. P. Moreno, that several distinct races of men existed in Patagonia before the arrival of Europeans. It is, however, still impossible to decide definitely whether any or all of these races lived in the country simultaneously, or whether they flourished successively at different periods. Some of the artificially deformed skulls are curious.

THE members of the energetic Field Naturalists' Club of Trinidad are continuing to publish valuable annotated lists of the fauna and flora of the island. We have just received the latest of these lists, dealing with the Reptiles and Batrachians, compiled by Messrs. R. R. Mole and F. W. Urich, with the assistance of Dr. O. Boettger and M. G. A. Boulenger. De Verteuil's work on Trinidad enumerates twenty-six reptiles, but the species of most of these are not to be recognised, except by the local names. Messrs. Mole and Urich are more scientific, and the list before us is only preliminary, to be followed by a series of notes on the life-history of the various species. There are six tortoises, twenty-five lizards, thirty-three snakes, and twelve batrachians, amounting in all to seventy-six forms, of which twenty-one are recorded for the first time. Dr. Boettger describes one new gecko (*Sphærodactylus molei*) and a frog (*Hylodes urichi*).

DR. W. H. DALL has been investigating the genus *Gnathodon*, Gray, a series of bivalve mollusca inhabiting subtropical America in the gulfs of California and Mexico, and preferring, like oysters,

brackish to either fresh or salt waters. By Gray and Desmoulins this genus was referred to Mactridæ, while the fact that in certain features it recalled Cyrenidæ was duly recorded. Fischer, in his "Manuel," under the synonym of *Rangia*, places it next to Cyrenidæ, at the same time raising it to family rank. A careful examination of the animal, however, has convinced Dr. Dall that the earlier naturalists were right, and the genus must be classed with the Mactridæ. Six genuine and six spurious or doubtful species are recorded in this monograph, which appears in the *Proceedings* of the U.S. National Museum, xvii., pp. 89-106; 1 pl.

Mr. R. W. Chapman and Captain Inglis are continuing their valuable observations on the tides of Port Adelaide, South Australia, and we have just received their brief report presented to last year's meeting of the Australasian Association. At that time they were proceeding with a second analysis of the Port Adelaide curves, with the aid of Professor G. H. Darwin's computing apparatus.

The lake formed a year ago by the great landslip at Gohna in the Himalaya, burst through its dam at the end of August, and thus has terminated one of the most gigantic geological catastrophes of recent years. Mr. T. H. Holland, of the Indian Geological Survey, has favoured us with a copy of his detailed illustrated report (*Rec. Geol. Surv. India*, vol. xxvii., pp. 55-65, with 5 plates and 2 maps), and from it we learn that the slip was due to the ordinary cause. The rocks on the side of the valley which collapsed dipped at a high angle towards the valley, and the landslip naturally followed heavy rains. On a small scale, as Mr. Holland points out, such valleys as that of Cheddar illustrate the same principle; the side on which the rocks dip towards the valley has a very gentle, stable slope, while the opposite side stands equally firm as a great precipice.

We are informed that a boring for coal will shortly be undertaken in the valley of the Stour. At a meeting held in Ipswich on the 28th August, the Chairman of the Directors (Mr. R. C. Napier) intimated that over £5,000 had been promised, so that there was no reason for further delay. It will be remembered that slaty rocks, older than Coal-measures, but of Carboniferous age, have been pierced at Harwich, and it is considered likely that coal basins may occur in the folds of the old rocks between the east coast and London.

Geologists of the rising school are continually attacking the orthodox theory of the origin of coal-seams; and it is quite likely they are correct in most cases, though the exceptions to the rule of growth *in situ* are never proved to be more than local. Another contribution

is made to the subject by Mr. Herbert Bolton in the last part of the *Trans. Manchester Geol. Soc.* (read June 12, 1894). He describes remains of plants and fishes from the Jarrow Colliery, Co. Kilkenny, Ireland, and concludes with an explanation of the well-known fact that a good deal of the anthracitic coal in this region does not rest upon the ordinary fire-clay, but upon a black shale. He considers that the coal thus situated represents the overflow of peaty matter from a lagoon in the neighbourhood, and thinks the case may be analogous to that of the bursting of a modern peat-moss.

For the past thirteen years Dr. Hermann Credner, Professor of Geology in the University of Leipzig, has contributed an important series of memoirs to the *Zeitschrift* of the German Geological Society, on the primitive batrachia and reptiles met with in the Lower Permian Formation near Dresden. The series being now apparently completed, he has just conceived the happy idea of presenting to those who have received the reprints a title-page and table of contents to preface the volume the memoirs will form when bound together. There is a list of eleven species of Stegocephali, mostly new, and the reptiles comprise the two important forms, *Palæohatteria* and *Kadaliosaurus*. Dr. Credner's very fine series of careful drawings are only rivalled by those of Dr. Anton Fritsch, in his well-known *Fauna der Gaskohle*, and the one work supplements the other.

We have received from Messrs. Dulau & Co. a catalogue of Zoological works, Mammalia, which contains some 2,500 entries.

I.

The Origin of Species without the Aid of Natural Selection.

A REPLY.

I AM much indebted to Mr. Wallace for his interesting paper (NATURAL SCIENCE, vol. v., p. 179). As he was the joint propounder of Natural Selection with Darwin, I could not hope for a weightier critic. Still, I am not in the least shaken in my opinion by it.

In reply, I would first observe that I take the terms "definite" and "indefinite"—which Mr. Wallace says he does not understand—from Darwin himself, who says: "The direct action of changed conditions leads to definite or indefinite results;"[1] while of the former he writes: "By the term definite action, I mean an action of such a nature that, when many individuals of the same variety are exposed during several generations to any change in their physical conditions of life, all, or nearly all the individuals, are modified in the same manner. A new sub-variety would thus be produced without the aid of selection."[2]

These words really strike at the root of Darwin's theory; and, indeed, the whole of my contention, if it were not founded on facts and observations, might be based on this passage; for Darwinism may be compared to an inverted pyramid, the apex being the *mistake* Darwin made in supposing variations in any seedlings of a plant (or variety) in *nature* being "indefinite." *They are always definite.* Though hundreds may perish, *the survivors all vary in the same direction*, viz., towards adaptation to the environment.[3]

In a correspondence with the late Professor Romanes last spring on this subject, he wrote me as follows: "Of course, if you could prove that indiscriminate [*i.e.*, indefinite] variations have not occurred in wild plants, but only under cultivation, you would destroy Darwinism *in toto*." (Hyères, March 12, 1894.)

Having stated my case thus briefly, I will proceed to remark upon Mr. Wallace's criticisms.

[1] "Origin of Species," 6th ed., p. 106.

[2] "Animals and Plants under Domestication," ii., p. 271.

[3] See, *e.g.*, "Origin of Species," pp. 72, 175, 176.

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Mr. Wallace writes: "It is, of course, admitted that direct proof of the action of Natural Selection is at present wanting." "At present"—why is it still wanting if it really exist? Has not one of the many biologists who have studied nature all over the world, during the last five-and-thirty years, been able yet to find one single proof?

On the other hand, I venture to say and to prove, *in the strictest sense of the term*, that Natural Selection is not wanted as an "aid" or a "means" in originating species.[4]

In the *elimination* of superfluous weaklings, in the *delimitation* of specific forms, and in the *distribution* of plants, Natural Selection may be largely credited with the results, but in the *origin* of species it is not wanted.

Darwin says that "Natural Selection has no relation whatever to the primary cause of any modification of structure"[5]; and the question with which I am solely concerned is to try and find out how and by what means variations in structure originate in plants; for new sub-varieties, varieties, sub-species, species, and genera are all based upon morphological variations; these being the only things systematic botanists trouble themselves with at all. *Then*, whether Natural Selection exists as a "means" or an "aid" in establishing these differences is a separate question altogether, as Darwin insists. To answer *this*, one looks to see, not only if Nature supplies those data upon which Natural Selection is supposed to act, but if they are of any use in the process. Mr. Wallace tells us what they are, for he says: "Offspring resemble their parents very much, but not wholly—each being possesses its individuality. This 'variation' itself varies in amount, but it is always present, not only in the whole being, but in every part of every being. Every organ, every character . . . . is individual; that is to say, *varies* from the same organ, character . . . in every other individual."[6] Now, is there any evidence, direct or indirect, that any such slight morphological differences as are here alluded to are of the slightest consequence to a *seedling* so as to enable it to survive in the struggle for life? What attempts have been made experimentally to test the truth or the reverse of this hypothesis?

Let it not be forgotten, too, that specific and generic characters are more often taken from the flowers and fruits, organs which are totally undeveloped when the "slaughter of the innocents" takes place, and, therefore, must be all put out of court so far as Natural Selection is concerned in bringing about the survival of the fittest. It has been suggested that a plant survives because, say, of some superiority in the structure of the flower, this feature being correlated with a more vigorous constitution than that of the other seedlings, which die in a premature state. I reply this simply begs the question,

[4] The title of Darwin's book is "The Origin of Species by means of Natural Selection."

[5] "Animals and Plants under Domestication," ii., p. 272.

[6] "On Natural Selection," p. 266.

or is putting the cart before the horse. *A seedling survives solely because it is vigorous.* This is capable of proof, and whatever flowers it may subsequently bear, it must be contented with them, whether they be the "best" or not for fertilisation or otherwise. In corroboration of the above, I would add my own experience with small and large seeds. These show that the better nourished have a much greater chance of starting and crowding out the rest by growing into larger plants, and that if small seeds be selected for some years, they either die out altogether or a tiny race of beings is for a time procured. Hence, for the word "fittest," *i.e.*, morphologically, I would substitute "strongest," *i.e.*, constitutionally.

I note here that Mr. Willis says (NATURAL SCIENCE, v., p. 240) that "Natural Selection has to be *dis*proved." No one, however, can be called upon to "prove a negative." It is for Darwinists to prove that the Origin of Species *does* really require the aid of Natural Selection.

On the other hand, it is for me to prove that the Origin of Species can take care of itself; in other words, to establish the truth of Mr. H. Spencer's observation: "Under new conditions the organism immediately begins to undergo certain changes in structure, fitting it for its new conditions,"[7] and that what is true for the individual is true for its offspring, the result being, to adopt Darwin's words, a new sub-variety without the aid of Natural Selection is produced.

I will now give illustrations of "definite" and "indefinite" variations. In 1847, Professor J. Buckman sowed seed of the wild parsnip in the garden of the Agricultural College at Cirencester. The seedlings began to vary, but in *the same way*, though in different degrees. By selecting seed from the best rooted plants, the acquired "somatic" characters of an enlarged root, glabrous leaves, etc., became fixed and hereditary; and "The Student," as he called it, having been "improved" by Messrs. Sutton & Sons, is still regarded as "the best in the trade." This is *definite variation*, according to Darwin's definition, for those weeded out did not differ from the selected, morphologically, except in degree, the variations towards improvement not being quite fast enough to entitle them to survive.

M. Carrière raised the radish of cultivation, *Raphanus sativus*, L., from the wild species *R. Raphanistum*, L., and moreover found that the turnip-rooted form resulted from growing it in a heavy soil, and the long-rooted one in a light soil.[8] Pliny records the same fact as practised in Greece in his day, saying that the "male" (turnip form) could be produced from the "female" (long form), by growing it in "a cloggy soil." Both forms are now, of course, hereditary by seed.

When a plant has been *long cultivated*, the relatively fixed nature, characteristic of most wild forms, generally breaks down; and the seeds from one and the same individual plant cannot always be

[7] "Factors of Evolution."

[8] This has been corroborated by M. Languet with the carrot. *Soc. Roy. et. Cent. d'Agricult*, 2nd ser., vol. ii., 1846–7, p. 539.

depended upon "to come true." Thus, an eminent agriculturist once said to me (a trifle hyperbolically, of course) speaking of the varieties of wheat: "You can almost get a different variety from every grain in a single ear."

Sir J. D. Hooker records no wild variety of the cabbage (*Brassica oleracea*, L.). Theophrastus (300 B.C.) only knew three cultivated forms. Pliny speaks of six, but who will count them now? It would seem as if plants underwent two courses of variation. First, in adaptation to it, by responding at once to a new environment, *i.e.*, definite variation. Then, when this has been thoroughly established, as with all of our ordinary vegetables, they may vary indefinitely, but why they do so no one can tell. Still, taking a broad view of the whole process, it is obvious that all such variations were primarily due to the environment of cultivation; because *they never occur in the wild state.*

Hence, to test the reality of specific characters of wild plants, as Mr. Wallace describes, by their degree of stability under cultivation in a garden, cannot possibly give but the most untrustworthy results. Some may resist for a time the influences of the new artificial environment, others may succumb to them; but it will be *the very best means of forcing them to change*; for, as Darwin and Weismann assert, cultivation induces variability. Suppose this test had been supplied to the wild and tall *Cineraria cruenta* with its small flowers; what would a systematist now say if he had never known the origin of the modern dwarf kind with large flowers of innumerable colours? He would undoubtedly call it a new species.

The rule may be laid down that *a species may be constant as long as its environment is constant, but no longer.* I have changed the spiny *Ononis spinosa*, L., the Rest-harrow, both by cuttings and by seed, into a spineless form undistinguishable from the species *O. repens*, L., in two years; but it would have, I doubt not, at once reverted to *O. spinosa* if I had replanted it in the poor soil from which I took it. It seems, therefore, to be a very hazardous and fallacious method of testing the value of specific or other characters by cultivation. A wild plant may or not change at once. Thus the carrot, *Daucus Carota*, L., proved refractory with Buckman, but not with Vilmorin, who converted this *annual* to a hereditary *biennial*, by sowing the seed late in the season, till the character of flowering in the second season became fixed.

Indeed, the proposed test is not unlike trying a man's guilt by making him eat an ordeal bean!

Mr. Wallace illustrates his remarks by the case of species of *Arabis*, but quite fails to perceive that it goes to prove my contention altogether. He says: "*A. anachoretica* has tissue-papery leaves—*due to its growth in hollows of the rock*" (my itals.). "Seeds of this plant, when cultivated at Kew, produced the common species *A. alpina*. The same thing occurs with many plants, as every cultivator knows."

If the rocky environment is to be credited with species-making in the one case, so must Kew be in the other. In both cases there is neither mention made nor need of any selection at all. Mr. Elwes told me that the various bulbous plants he introduced from the East into his garden at Preston, Cirencester, changed so greatly in a few years in all their parts that he could scarcely recognise them again.

Mr. Wallace adds: "Other forms, with no greater peculiarities externally, preserve their characters under cultivation, though exposed to the most varied conditions."

This is equally and quite true; but any investigation into the causes of the origin of species by *variation* has nothing to do with any other question of the causes of preservation of the type-characters, or *heredity*. Evolution accounts for all living beings by variation; but it does not attempt to offer any explanation of the existence of "survivals." *E.g.*, *Nautilus* and *Lingula* have lived on from the Silurian days till now; *Equisetum* has flourished from, at least, the Carboniferous epoch till to-day. Therefore *change is not absolutely necessary in organisms under changed conditions;* but when it does occur, then I maintain, with Dr. Weismann, that all changes are primarily due to external influences. He says: "We are driven to the conclusion that the ultimate origin of hereditary individual differences lies in the direct action of external influences upon the organism."[9]

Mr. Wallace is good enough to call attention to my book, "The Origin of Floral Structures by Insect and Other Agencies,"[10] and attacks, very rightly, what I fully admit may be regarded as a weak point in it; *i.e.*, I can bring but few positive illustrations to demonstrate my view that irregular flowers have been formed through the direct action of insects from regular ones; but he quite ignores the whole line of argument running through the book in support of the probability. It is one which Dr. Weismann recommends in support of evolution, which "may be maintained with the same degree of certainty as that with which astronomy asserts that the earth moves round the sun; for a conclusion may be arrived at as safely by other methods as by mathematical calculation."[11] It is the well known argument of the accumulation of coincidences which can furnish probabilities of so high an order that they may be regarded as an equivalent to a demonstration. Thus, physicists tell us that they *know* the composition of the sun, but their knowledge is solely based on the coincidences between the lines of the solar spectrum and those of vapourised substances.[12] Similarly with flowers: when we find innumerable coincidences all tending in one direction, coupled with an indefinite capacity for varying in response to forces in all parts of

[9] "Essays on Heredity," etc. Eng. trans., p. 279.

[10] *International Scientific Series*, vol. lxiv.

[11] "Essays on Heredity," etc., p. 255.

[12] The "fact" that udders have become enlarged by hand-milking is based on a similar accumulation of probabilities.

plants, I still maintain that "Mr. Henslow's theory [does not] utterly break down." Mr. Wallace contends that the negative evidence derived from "regular" flowers, as gentians, tells against me, as they ought to have long ago become irregular, since their "lower petals have been always subject to irritation and have never developed irregular flowers." This is scarcely fair; for not only do all botanists believe—on precisely the same grounds of probabilities—that all irregular flowers have descended (somehow) from regular ones; but that, if he will refer to the chapter on "Peloria," he will see that existing regular flowers, being mostly "terminal," have no "lower" petals at all, but are so situated as to offer access to insects from all points of the compass. Moreover, whenever a plant with normally irregular flowers (which are always situated close to the axis, so that insects can only enter them in one way) produces a blossom in a terminal position (as foxglove, larkspur, horse-chestnut, etc., often do), it at once becomes quite regular. These differences between regular and irregular flowers represent two of those groups of coincidences respectively, to which I referred.

Mr. Wallace adds: "The very first essential to this theory is to prove that modifications produced by such irritations are hereditary." Quite so. But this proves itself, if my contention be right; for plants with irregular flowers *are* all hereditary. So that there is no need to prove this point, provided the "previous question" as to the origin of irregular flowers themselves be answered. But the converse change can be readily shown; for flowers, normally irregular in nature, often revert to their ancestral regular form under cultivation in the absence of insects, and then come true from seed, as do Gloxinias. Unfortunately, one cannot make a regular flower become irregular. How long it required in nature to do so no one can tell; but all the innumerable minute details of structure coincide to one end; a multitude of correlations all fit together for one effect; so that we may put the alternative thus—Which is more likely, that some one common cause has set up these minute, often microscopic, details in unison together; or that they have arisen by selection out of innumerable wasted variations, which no one ever saw in nature, nor can even ever see a trace of under cultivation?

When, however, we come to variations in the vegetative system of plants, there is nothing easier than to prove, first, the direct action of the environment, and secondly, the hereditary persistence of the result. I need go no further than to take Buckman's parsnip, Carrière's radish, Vilmorin's carrot, or anybody's variety of cabbage. What are all these and many other instances but experimental verifications.

Mr. Wallace alludes to my last paper on "The Origin of Plant-Structures by Self-Adaptation to the Environment, exemplified by Desert and Xerophilous Plants,"[13] and attacks my inferences with

[13] *Journ. Linn. Soc. Bot.*, xxx., p. 218.

regard to spinescent processes of desert plants; but he again ignores the primary argument of innumerable coincidences; while in the case of vegetative organs this argument has been in many cases "verified by experiment." When, however, Mr. Wallace calls in question my statement that spines are correlated with a dry soil and atmosphere, he controverts those of Belt, Aitchison, Scott Elliott, Grisebach and others, for he says: "There is no such general coincidence of aridity of soil and atmosphere with abundance of spiny plants, as very little enquiry will show." Having seen and gathered them myself in the Libyan desert and even on our own sandy heaths, I cannot accept this statement; and if those eminent travellers I have named are misleading us, where are we? He then mentions the Galapagos and other islands, where, though of a desert character, plants are *not* spinescent. Here, again, I am not concerned with what does *not* occur, but with what *does.* Moreover, any cause that may tend to arrest an axis likewise may tend to render it spinescent, and more than one cause may produce the same result,[14] so that it is not altogether strange to find spinescent processes away from deserts; but I *do* maintain that spinescence is one and an important element in the *facies* of hot and arid deserts with a barren soil.

Mr. Wallace advances the well-worn theory of the interaction of mammals and spines. In the first place, if I may still believe in the prevalence of spines in deserts, they occur where no herbivorous quadrupeds live. Secondly, if a mammal wishes to eat a spiny plant, it somehow often gets over the difficulty; thus donkeys knock off the spines of *Opuntia*; horses eat gorse. I had a cow which was partial to holly, another rejoiced in nettles! But all this is beside the question. It seems to me that there is a lurking element of teleology in this view: for any structure which arises *in anticipation of its use* savours of natural theology[15] rather than of evolution by natural processes alone. I fully admit that plants, when once they have got their spines, may be able to keep animals more or less at bay; but they originate, I maintain, as a mere accidental and inevitable result of an arrest of the organ in question, such arrest being mainly due to drought.

If teleology in its old dress of *Design in anticipation of Use* is, and ought to be, extinct, we may accept Darwin's form of it, that Evolution is the Deity's method of creation. Let us, then, recognise protoplasm as having been impressed with the power of self-adaptation—such being the inference from direct observation of its behaviour; and, consequently, enabled to build up structures in an automatic response to the environmental forces, whenever it is necessary to bring about a better degree of equilibrium between the internal and external forces.

[14] I observe Mr. Osborne makes a corresponding statement. NAT. SCI., p. 223.

[15] Indeed, such anticipation is absolutely necessary for the theory of Natural Selection in general.

On the last page but one of his paper, Mr. Wallace alludes to the case of the hard shells of nuts, and asks if the direct agency of birds, monkeys, etc., has anything to do with them. He admits the question is absurd. I do not therefore know why he asks it. I have not myself written a line on this branch of the subject, but will suggest, from what one knows of all other parts of plants having the capacity of varying, that I see no reason for inferring that hard coats of fruits should be subject to any different law. Soft fruits vary readily enough, as melons, pea-pods, apples, as well as pears in their degrees of "stoniness." Moreover, under cultivation, varieties of forms of nuts and walnuts have arisen, as well as of olives, almonds, and dates, and other hard-coated or hard-seeded fruits. The fact seems to be that cultivation affects the whole organisation of the plant; for the environment is not always solely concerned with an isolated bit of a plant, as a nut or a root. Many visible changes are due to secondary causes within the individual; but in all cases, as I believe with Dr. Weismann, they are *primarily* attributable to the direct action of the environment, simply because *they never occur unless the environment itself is changed.*

Finally, to return to my starting point. The whole question lies within a very small compass. Thus, first, no one disputes the fact that the environmental forces can act upon an organism. Secondly, that the organism can respond to those forces. But now follow two views. Darwinites say that the resulting variations are indefinite in Nature, just as they so often are in cultivation; and that the environment *selects* the best fitted *to survive.* I say that they are always definite in Nature: and not only exceptionally so, as Darwin thought; and that the environment *induces* the best fitted *to arise.*[16] Therefore, Natural Selection has nothing to do in aiding the Origin of Species.

For additional facts I would refer the reader to a paper entitled, "A Theoretical Origin of Endogens from Exogens, through Self-Adaptation to an Aquatic Habit"[17]; and to a companion volume to the "Origin of Floral Structures," which I hope will be shortly published in the "International Scientific Series," and entitled "The Origin of Plant Structures by Self-Adaptation, in Response to the direct Action of the Environment." In this, similar lines of argument, with illustrations, will be applied to Desert, Aquatic, Maritime, Alpine, and Arctic, as well as Climbing Plants, and to the Origin of Peculiarities of Roots, Stems, and Leaves.

George Henslow.

[16] See "Animals and Plants under Domestication," ii., p. 272.

[17] *Journ. Linn. Soc. Bot.*, xxix., p. 485.

II.

Horticultural "Sports."

THE immense variety existing at the present day in the cultivated flowers, foliage plants, fruits, etc., which figure in our horticultural shows, while evoking the admiration of the many, is rarely appreciated at its true value as the outcome not only of infinite labour in culture and careful selection, but as largely due to vagaries and freaks of nature quite outside the scope of any known laws. Thus, we see flowers such as tulips, hyacinths, dahlias, and others exhibiting all, or nearly all, the colours of the rainbow in conjunction with most complex variations in the form of the blossoms, all of which have been developed from single-flowered normal wild plants, with one-coloured petals. Now, a large part of this development is undoubtedly due to the careful selection and accumulation, generation after generation, of comparatively small differences which appear in the seedlings, but it is a remarkable fact, of such general occurrence as almost to constitute a law, that when a wild plant is brought under culture and thus subjected to more or less artificial surroundings, it becomes in some way subtly modified, so that not only is its offspring likely to vary more widely than before, but even individual buds of the plant itself may produce modified foliage, flowers, or fruits capable of being propagated and so forming a new and permanent variety. Probably the most striking instances of this are seen in the origin of Nectarines and their varieties. These have all sprung direct as perfect nectarines from peach trees or peach stones. Old peach trees, after producing peaches only for many years, have produced fruiting branches bearing true nectarines, and since nectarine stones in their turn may yield true peaches, it would seem that it is really a case of dimorphism, akin to the two forms of leaves sometimes seen in plants. This phenomenon has repeatedly occurred in the peach, most of the many varieties of which have been raised from stones, though some have originated by buds, and it is curious that in a few of these cases the fruit has only varied in the direction of being earlier or later in ripening—a feature which is constant. A well-authenticated case is recorded of a gooseberry bush being so sportive in its buds that four widely different sorts were produced upon it. Cases of this description, however, bring us to the question of cross-fertili-

sation, by which means the horticulturist often succeeds in blending or modifying the colours and forms of two very different varieties or even species, one result of which is a tendency to revert, and the gooseberry in question may have owed its diversity to this. The tuberous Begonia, for instance, which now forms one of the most salient features of our shows, began its upward career with a blend of two distinct species. This blend, in its first results, yielded plants of very different habits to either parent. Fortunately, the alliance was a fertile one, hence propagation by seed and selection of offspring presented no difficulty. It was speedily seen that the two species were sufficiently at variance in the blood of their joint progeny to induce an immense divergency of type, and seed being sown from the most diverse forms, the final result was that no two of a batch were quite alike. Here, of course, was the cultivator's opportunity, and in the hands of Mr. J. Laing and Mr. H. Cannell, in the course of twenty years most magnificent strains of huge double and single flowers, of all habits and all tints of white, salmon, and red, have culminated in constituting the naturally insignificant begonia a fair rival to the very Queen of Flowers herself, barring her perfume. Here is, indeed, a triumph of Art over Nature, so far as the art of selection is concerned.

What, however, the public do not see, though they feel it in the high prices necessarily demanded for the results, are the thousands upon thousands of worthless plants which accompany the good ones. Often and often, and especially in the earlier stages of development, the cultivator has to raise to blooming point, or, worse still, in the case of trees, to fruiting point, an enormous number of seedlings in which he may vainly search for a "break" in the right direction. This "break," obtained, however, he starts under ever-improving auspices, until—as in the case of Begonias in question—he finds his best flowers barren, their extreme development being at the cost of their reproductive energy.

Then, too, it is a rare thing indeed for the breeder to be perfectly satisfied. Frequently, when his flower is perfected as regards the blossom beyond his utmost expectation, the stalk is too short to display it properly or too weak to keep it upright. A heavy double flower with a pendulous habit will not do. It must look you boldly in the face and display its charms effectively. In time this is generally arrived at by judicious crossing, or it may be by simple selection of seedlings, provided always the utterly barren stage has not been reached.

Fashion plays a large *rôle* in horticulture. Dahlias, for instance, starting from a simple single starlike bloom were transmuted by long years of selection into huge spherical masses of regularly folded petals, and decorated with any colour desired except the unattainable blue. Suddenly the æsthetic craze steps in with the sunflower as its floral deity, and lo! the dahlia must be single and the cultivators

have an opportunity of marketing their wastrels, at the expense, of course, of their choicest. Then the Juarez or Cactus type turns up from Mexico, and becomes the rage, and in a season or two we find it evolved in all tints, and largely from the old forms, many being christened Cactus, which, in the breeder's opinion, had simply "gone wrong," and, but for the fashion, would have decorated the dust-heap.

So far, we have touched mainly on the minor "sports," which need to be accumulated to be striking; but in many cases, as in that of the nectarines, among the fruits we owe some of our best flowers to wide and sudden departures from previous types. The beautiful double crimson hawthorn, for instance, appeared as a bud-sport on a pink one, a branch resulting which was entirely covered with dark crimson flowers. Many of our best roses we owe to bud-sports. The white moss-rose came as a sucker on the red, and, when severed for propagation, the striped moss-rose sprang from a bud near the cut. One variety of chrysanthemum yielded, in the same way, no less than six distinct forms. Generally, varieties originating in this manner remain constant when propagated by cuttings, but are very liable to revert when reproduced by seed. Variegated forms innumerable have originated as bud-sports on normal green plants, and these, when cut off and propagated by buds, retain their peculiarities. The writer recently saw a plumose form of *Polystichum angulare* of which six fronds out of seven were profusely splashed with pure white, yet the plant was several years old, and neither it nor its parent, of which it was an offset, ever showed the slightest tendency to variegation before. This case is very peculiar, so much of the plant being suddenly affected, and to such an extreme extent. Seeds of variegated sports of this class generally produce normal green and wholly white plants, the latter of which are very short-lived, owing to the absence of chlorophyll. In very rare cases, a graft has been known to affect the stock, and form a graft-hybrid. *Cytisus Adami*, a hybrid laburnum, is, perhaps, a unique oddity in this way, being a quasi combination of a purple and a yellow laburnum. The leaves of each species differ materially, as does the general habit of growth, and in specimens of this, which may be seen at Kew, the tree is constantly sending out branches and twigs of each distinct species side by side, and mixed up in the most curious fashion; the foliage and habits peculiar to each accompanying the characteristic bunches of yellow or purple flowers.

The well-known Cockscomb, a densely fasciated form of *Celosia*, is ordinarily a rich crimson, but at a recent show of the Royal Horticultural Society, one grower who had made a hobby of this for years exhibited fifteen distinct colours of it, including a yellow, and every plant was furthermore crimped and crested in a fashion of its own. Patience is indeed a virtue to the selective cultivator. Tulips, to wit, frequently remain for many years in their "unbroken" stage. The seedlings of this family, when they first bloom, present very different

characters from the parental form, but after a time they "break" or assume their proper character quite suddenly, and until this occurs it is impossible to say whether they are valuable or not. Orchids owe their vast variety chiefly to unaided Nature, and the efforts of skilled travellers who ransack their native habitats to find new ones. Of late, however, much has been done by crossing allied species, and so obtaining flowers embodying their united charms. Imported Orchids form somewhat of a lottery, and many a prize has been acquired unwittingly in the shape of a dormant pseudo bulb, costing a few shillings, and fetching, when its true character has appeared in its bloom, a handsome sum in guineas. Such a case occurred not long since, when an orchid was put up in a thumb-pot, with a tentative reserve of a few pounds, and was knocked down, after a spirited competition, at one hundred and forty guineas. Such plants may, of course, be new species, but usually they are varietal forms of well-known ones, and are thus simply natural "sports" of precisely the same character as those already described.

CHAS. T. DRUERY.

III.

On the Geology of the Plateau Implements of Kent.[1]

THIS subject has been fully treated by Professor Prestwich in his several memoirs in the *Quart. Journ. Geol. Soc.*, vol. xlv., 1889 (the old worked flints at pages 282–286); vol. xlvi., 1890 (the general geology of certain drift-deposits, and the geological stages of their formation, pages 166–168 and 179); vol. xlvii., 1891 (the same subjects, pages 126–159, particularly pages 157–159, and plate 6, and the peculiar flints at page 160, with plate 8). Also in the *Journ. Anthrop. Instit.*, vol. xxi., 1892, pages 246–270, with plates 18–21.

Mr. W. J. Lewis Abbott has more lately contributed to NATURAL SCIENCE (vol iv., no. 20, April, 1894) an interesting paper "On the Plateau Man in Kent," containing a clear account of the old rude implements found on the Chalk Plateau, illustrated with two plates.

It is shown in these memoirs that certain superficial soils on the North Downs, between Sevenoaks and Rochester, contain numerous rudely-worked flints, which are connected with a gravel area, of very great antiquity, and probably derived originally from the side of the old Wealden Range, which once rose 2,000 to 3,000 feet above, where Crowborough and other hills in Sussex now are. Those heights were long since removed by natural agencies, such as rain, rivers, sea, frost, and ice, probably with more than one time of energetic action, and portions of their *débris* were distributed by old streams or otherwise to and over a plateau of Chalk at a lower level on the flanks of the range.

These rude old implements have an ochreous tint, and are associated with limited patches of much-worn gravel having the same colour, and not forming well-defined beds, by no means thick enough for the making of a gravel-pit. Nor are the implements under such conditions as would lead to the supposition of their having received

[1] This paper was read before a combined meeting of the Anthropological and Geological Sections of the British Association, on August 10, 1894, in consequence of a suggestion made by the Anthropological Institute that it was desirable to know more of "The evidence afforded by the 'Plateau Gravels' relative to the Antiquity of Man."

their staining in their present position. A few only have been found beneath the surface.

It is very desirable that excavations should be made at proper places on the plateau to discover, if possible, the extent and thickness of the implementiferous soil.

The peculiar reddish-brown colour of these rude implements, with ochreous stain and some adherent particles of limonite, was probably obtained when they were lying in an old ferruginous gravel in their original place; and some of this gravel has accompanied them to the plateau. Their colour and greater wear distinguish them from the few palæolithic and neolithic worked flints found on the plateau. Some have a strong resemblance to the "valley forms," and were most probably transitional in character and condition. Many are very much water-worn—an indication of their long and distant travel. Some are *scratched*,[2] by very close pressure, either with or without the intervention of ice-action.

The evidences borne by these peculiarly-shaped flints of their having been fashioned by the hand of Man, have been carefully dealt with by Professor Dr. Prestwich (see above), Messrs. B. Harrison (their discoverer), De B. Crashaw, W. J. Lewis Abbott, A. Montgomerie Bell, and others. Mr. J. Allen Brown has succinctly referred to them as "Eolithic: roughly-hewn pebbles and nodules, and naturally broken stones, showing work, with thick ochreous patina; found on the plateaux of the Chalk and other districts, in beds unconnected with the present valley-drainage"; and "under conditions which clearly indicate that they are older than the usual valley-drift implements." (*Journ. Anthrop. Instit.*, vol. xxii., 1892, pp. 93, 94).

In "The Ground Beneath Us," 1847, pages 71–79, many interesting points bearing on this subject were already illustrated and explained by Professor Prestwich. The great changes of land-surface in this part of the European area were referred—firstly to the movements, accompanying the elevation of the Pyrenees, when the Chalk became dry land, with its uplands, valleys, and estuaries; and he showed that at that time the Wealden area formed an island in the Thanet-Sands Sea.

Some rivers afterwards brought down the clays and sands which now constitute the "Woolwich and Reading" beds. With some submergence other Tertiary beds were formed, probably extending over a part of what is now the Wealden area.

Secondly, this area, with its stratified coatings, was raised up (after the formation of the London Clay) by movements accompanying the elevation of the Alps by the lateral pressure caused by the earth's contraction. The other Tertiary beds (of Paris, Bracklesham, etc.) had helped to shallow the open sea surrounding the island of the

[2] On one and the same specimen are some scratches quite raw, and some that have been coloured with red cchre, also some merely linear streaks of red hæmatite.

Weald, which was ultimately to be an elevated, elliptical, weather-worn, and sea-eaten dome of great height.[3]

The successive stages were :—

1. When the elevation of the Wealden area attained its maximum, there was certainly a considerable thickness of Chalk on the surface, and this was necessarily exposed to marine and atmospheric denudation.

2. The immediate result of this was the wearing-away of the

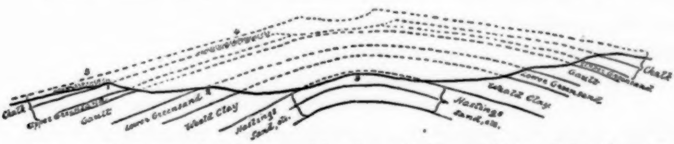


FIG. 1.—Diagram showing roughly the relative position of the formations constituting the Wealden Anticlinal between the North and South Downs (1. Limpsfield, 876 ft. ; 2. Oldbury, 620 ft. ; 3. Crowborough, 803 ft.) ; also their successive denudations ; and the original place of the Old Gravel (4), some of which was brought down by natural agencies to the Chalk Plateau (5) now existing.

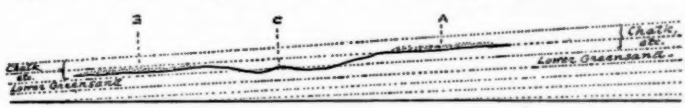


FIG. 2.—Diagram showing the possible position of the Lower Greensand outcrop (*C*) when the Old Gravel (*A*) was being transferred from the higher (*A*) to the lower (*B*) level of the Chalk by natural agencies along a continuous surface. The strata are set at too high an angle in Fig. 1 to show what is required here.

(*A*) Old Gravel in place. (*B*) Plateau Gravel derived from the Old Gravel.
(*C*) Outcrop of the Lower Greensand.

Chalk, and the trituration of the washed-out flints; and thus the formation of a great bed of shingle—that of the Thanet Sands.

3. With continued wave-action, these shingle-banks were washed away and distributed at a lower level, to be the Pebble-beds of the "Woolwich and Reading" series, which had been formed in the meantime by rivers from the hill-ranges. The pebble-beds can now be seen at Addington, Blackheath, etc., extending to the very edge of the present Chalk escarpment.

4. The deposits of this stage were next removed in part ; and in course of time the Diestian beds were laid against the flanks of the lowered and perhaps sinking range. Of these strata some limited patches, such as the Lenham beds, remain here and there on what are now the Chalk Downs.

5. One or more accumulations of Chalk-flint *débris* were formed

[3] A neat little diagrammatic section across the Wealden area, showing the high elevation of the anticline (about 2,000–3,000 feet ; Topley, *Mem. Geol. Surv. Weald.*, 1875, p. 217), was published in Professor (Sir A.) Ramsay's "Lectures on the Physical Geology and Geography of Great Britain" (five editions, 1863–1878). At the time of this elevation of South-eastern England, the Ardennes attained a height of about 18,000 feet, as shown by MM. Cornet and Briart, *Mém. Soc. géol. Belgique*, vol. iv., p. 71.

at about this stage of the history of the old Wealden Range or Island, and were probably characterised by the presence of iron-oxide in greater quantity than in the common ferruginous gravels of the South-east of England at present. Man, being present, used such pieces of the flint as suited his requirements. Probably, at first, with little or no alteration of their form; but afterwards he applied them with definite modification of their shapes to meet his wants in killing, skinning, cutting, fire-making, rubbing, pounding, scraping, drilling, knocking, breaking, chopping, digging, etc., that is, in tooling and other processes. Such implements he made and left there, on that old, very old, probably pre-Glacial ground. (*See* Fig. 1.)

6. This gravel extended down the side of the "dome," perhaps tailing down the slopes of uplands by slips, slides, and slushings, probably by more than one stage, after its formation; or, during a succeeding age, the encroaching sea cutting away the lessening dome, or torrents scoring the hill-sides, removed the more or less extensive deposits of ferruginous gravel, with the rude implements left upon it, and spread out the much-worn relics on the slopes of the Chalk below. Here they are now found on the isolated plateau; and they lie on the red Clay-with-flints, that had been in process of formation previously, for ages, by the gradual solution of the Chalk below, and the settlement of argillaceous and sandy matter from the overlying and gradually disappearing Tertiaries. This was co-extensive with the Chalk-surface; and on it lies some of the transported ochreous gravel, together with Tertiary pebbles, less-worn flint-stones, and some *débris* of the Lower Greensand, which the wave-line had then reached. The presence of chert fragments from the Lower Greensand proves that the current of driftage (or the tailing of the gravel) must have passed over the outcrop of the Lower Greensand, and therefore here from south to north, on a continuous surface. (*See* Fig. 2.)

7. Subsequently the outlying Chalk (now the plateau above referred to, sloping from an elevation of about 800 feet on the south, to 400 feet, and less, on the north) was cut off, by denudation in the Glacial Period, from the remaining uplands of the once lofty range; the Holmesdale (or Weald-clay Valley) lying below the escarpment of the Lower Greensand at the foot of the diminished dome, and the Gault Valley at the foot of the Chalk escarpment. (*See* Fig. 1.)

The Diestian or Lenham beds were formed in the early Pliocene period, and the denudation probably began directly afterwards, at about the time of the Red or the Chillesford Crag, in late Pliocene or post-Pliocene times; and as the old ferruginous gravel had not only been formed but had been brought to a lower level before that time, it must be regarded as of pre-Glacial age.

A similar series of occurrences and geological results evidently took place on the south side of the old Wealden uplands, giving origin to the brown-coated rude implements at Friston, near Eastbourne, in Sussex.

The probably great changes of climate, due to rearrangements of land and water and of high and low ground, form part of Professor Prestwich's interesting history of the Wealden hill-ranges, in the sculpturing of which not only aqueous, but glacial agency must have had a large share.

It must have been a great pleasure to the veteran geologist, Professor Dr. Prestwich, to find that his conclusions (in 1890) as to the Pliocene Tertiaries and gravels on the flanks of the diminishing island of the Weald fitted so truly, as consecutive history, with his early views (1847) of the probable conditions of the Wealden dome in Eocene times.

Perhaps it may be asked :—

I.—Why may not the Rude Implements belong to the Red Clay-with-flints, since some few have been obtained in shallow diggings on that bed?

The reply is :—1. Because that clay was not ever moved about, much less *drifted.*

2. Because that clay occurs over other and larger areas without containing any such implements, but only unrolled flints separated from the Chalk by its dissolution.

3. Because the implements are accompanied with chert and ragstone from the Lower Greensand, which *débris* must have come from a higher level—namely, from the outcrop of the Lower Greensand on the side of the hill-ranges of the "Dome," *before* the denudation of the present Gault valley.

4. If the Implements had *settled* down from above *with* the "Clay-with-flint," they must have *either* been in the Tertiaries (!) to be let down, as the Chalk dissolved below (possibly when the Chalk had a somewhat higher slope against the "Dome"),—*or* they must have *been on the* Chalk *before* the Tertiaries were formed there!!

5. The "Red Clay-with-flints" is of local origin, whereas the rude implements, with the sprinkling of brown gravel, are foreign to the place of their occurrence. And on the surface, where these implements are found, chert and ragstone from the Lower Greensand always occur.

6. Professor Prestwich informs me that he has examined hundreds of sections of the "Red Clay-with-flints," and has never found the implements in it. Some of the reputed cases of their presence in this clay are doubtful. In other cases, mentioned by Mr. Harrison (see the list on next page), the depth is so small that the implement may have worked itself down, or may have fallen into a hole.

7. The flints in the red clay are not stained. The crust of the flint retains its white colour; or, if the flint has been broken, the surface of fracture has weathered white. The red clay does not stain; it seems to have removed the water-of-crystallisation, and left a white surface on the flint.

8. Thus both the travel-worn gravel and implements must necessarily have had their source in the flints of Chalk situated at some *higher* level.

II.—Why not belonging to a *local* ochreous drift, deposited originally on the plateau?

The reply is:—1. Because the plateau is out of reach of any present (or late) water-level.

2. Because there is too little of the ochreous gravel there to constitute such a local drift.

NOTES.

1.—The old brown "plateau implements" are not found beyond the plateau, except in a few instances when they have been derived from the wear of the plateau, and probably this may be said also of other plateaux.

2.—As to these being *natural* forms, where can any such forms be shown to occur in a river-gravel?

3.—The extremely worn condition of the ochreous flints and implements indicates long-continued movement and great water-wear, if not violent transport; and no well-marked, definite deposit of such a gravel occurs. The tail of the old deposit on the Dome, however, may have extended from the original slopes down to the Chalk level (now the plateau), by more or less violent slides and *débacles*, before the bounding or dividing valleys were excavated in the Glacial Period.

The following is a list (from letters) of the Rude Implements found *in situ* by Mr. Harrison. For the localities, see the Map, *Quart. Journ. Geol. Soc.*, vol. xlv., pl. 9, and *Journ. Anthropol. Instit.*, vol. xxi., pl. 18.

1.—One flake, rude.—From an old sand-pit (now a pond) at Ash.

2.—Three rude implements.—From a trench nearly three feet deep, in sand, with green-coated flints (probably a remnant of Thanet or Woolwich-and-Reading Sands), at Ash.

3.—Two rude.—From a trench in similar sand, 2½ feet deep (for laying in mangold wurzel), at Ash.

4.—Two rude.—From deep-red clay at the Vigo, two finds with an interval of two years.

5.—One rude.—From a hole (for finger-post) in red clay at the Vigo Inn.

6.—One rude—From red clay thrown out in digging a grave at Cudham churchyard.

7.—Several rude.—From trench (for water-pipes), crossing a patch of ochreous gravel on red clay (?), at Dunstall Hill, Shoreham, Kent; three tips of neoliths (?) and many rude implements, all water-worn.

8.—One rude (?).—From a post-hole, 2½ feet deep, at Kingsdown.

9.—One rude.—Fine large implement, at 2½ feet, Ash.

10.—Several rude.—From a deepened dew-pond, near the 775 feet summit level; about thirty rudely-worked flints, and some Oldbury Stone (Lower Greensand).

11.—Some rude.—From a large pipe in the Chalk, filled with deep-red clay, at 720 feet, in an old Chalk-pit, a little below the crest of the escarpment, some deep-tan-coloured implements.

12.—Some rude.—From the grave for a horse on the crest of the escarpment, at about 775 feet; the surface soil was opened out for five feet, and "old implements" were found in the clay and gravel thrown out.

Hence it can be seen that there is evidence of the existence of the Rude Implements locally beneath the surface, and that systematic excavation is required.

T. RUPERT JONES.

IV.

The Effect of Temperature on the Distribution of Marine Animals.

THE question of the influence of temperature on marine animals is a comparatively old one. Twenty years ago and more it had been already discussed in England and America, but even at the present day it is far from being solved. On the contrary, very conflicting views prevail as to the importance of temperature on the distribution of marine life. Some authors regard it as the sole important factor, others do not attribute any value to it at all. This diversity of opinion is less the result of a lack of facts, our store of which has been greatly increased by the last years' expeditions; but it is rather to be attributed to an incorrect grouping of the facts. My intention, on the present occasion, is not so much to bring forward new data as to re-arrange those we already possess.

The conflict of opinion may be seen in the work of two investigators, whose particular line is geology, but who have occupied themselves especially with marine biology as a territory intermediate between the sciences of Zoology and Geology. I refer to Walther and Heilprin.

Walther says, at the end of a detailed argument, after enumerating many facts, which are to prove the influence of temperature, that the latter is not only *one* factor, but the exclusively determining one, in comparison to which all others, like light, atmospheric pressure, etc., are quite in the background.

Heilprin, in a discussion of the fauna of the deep sea, does not attribute so great an influence to temperature, and brings forward the following train of argument. Reef-building corals, which flourish best at 70–75° F., and which do not grow below 68°, would find this temperature in many oceanic parts, even at a considerable depth, in the Red Sea, for instance, even down to the bottom; nevertheless, they occur everywhere only at the surface. Conversely, the fauna of the deep sea begins in the lesser depths (100 fathoms if there is a bottom), and, though the temperatures are much higher there than in the real abysses, the fauna is the same. The beginning of the fauna of the deep sea is always to be recognised at a certain depth, which is about the same in metres everywhere, but which must have a very

different temperature in the different seas and different latitudes. Moreover, if temperature is the determining factor, Polar animals ought to be the same as abyssal ones in lower latitudes, which is denied by Heilprin.

In the whole of this argument no clear distinction has been made between three classes of facts and relations :—

1. In the first place, no distinction has been made between pelagic animals, the Plankton and the Benthos, *i.e.*, the life confined to the shore or ground, animals either sessile or capable only of creeping, in contrast to the Nekton, the freely-swimming animals, capable of considerable movements, able to stem the tide and migrate at will.

2. Secondly, without further consideration a parallel has been drawn between the horizontal and the vertical distribution with reference to temperature, while the latter becomes complicated by other factors, too, as pressure, want of light, etc. Moreover, the gradations in the change of temperature in different depths are not all proportional to the amount of the depths themselves.

3. In the above argument no distinction has been made between eurythermal and stenothermal animals.

A few words ought to be said to explain these terms (first adopted by Moebius). We might give a definition by saying that eurythermal animals can bear great differences of temperature, stenothermal animals cannot, and this can be represented in a graphic manner. Each animal has a certain optimum of temperature, at which it flourishes best. A number of degrees above it its metabolism cannot go on; and in a similar manner, at a certain degree below this optimum, the metabolism ceases also. Thus we obtain for each animal three points in the thermometrical scale—optimum, maximum, and minimum. Now in some animals the space on the thermometrical scale between maximum and minimum can be very great : these are eurythermal animals. In other cases it is small : these are the stenothermal animals.[1]

It is clear that eurythermal animals cannot be made use of either as a proof or a contradiction of the influence of temperature. Many animals of the shore, especially molluscs and echinoderms, are proved by experiments to be eurythermal. If a bivalve, for instance, is found both in the North Polar Sea and in the lower strata of the German or English Sea, nothing is proved *for* the influence of temperature—just as little as the occurrence of a eurythermal animal at the surface both in the Mediterranean and at Spitzbergen would be a proof *against* it. This has often been overlooked.

Moreover, it is easy to understand that it is necessary that the

[1] It is obvious that warm blooded animals can bear greater differences of temperature because they have their mechanism of heat regulation in their own body ; they are therefore *eurythermal*. The poikilothermal animals, on the contrary, to which the animals of the sea belong almost exclusively, are more stenothermal *à priori*.

animals of the Nekton, *i.e.*, the animals which swim well, should not be too sensible to temperature; for by their power of moving either in a horizontal or in a vertical direction, they pass easily into very different conditions and have to be adapted to them, if the faculty of swimming is to be of any use to them at all. As a matter of fact, we find that the large swimming animals, such as crustaceans and fishes, are, in general, eurythermal.

The geographical distribution of whales seems at first to contradict this consideration, but in reality it does not. For if we find some of their species chiefly inhabiting the high north or south latitudes, it is not because they are unable to endure higher temperatures, but because only in those regions can they find the enormous quantity of organic substance, of Plankton, which they require for their gigantic bodies.

Here we touch another point of importance. In high north and south latitudes, the production of Plankton, the primordial food, the algæ, the small crustacea, etc., is extraordinarily great, because the variations of temperature are here small, and therefore the life-conditions are very favourable, in spite of a low degree of temperature. This also has contributed to obscure the question of the influence of temperature. If an animal cannot exist below or above a certain degree of temperature, it is clear that the determination of the *average* temperature of a place is of small zoögeographical value; for this average temperature can just as well be the mean between two temperatures suitable for the animal as between two which are harmful or fatal to it. The attempt has therefore been made to draw lines of greatest cold as decisive for the question, and these are termed isocrymal lines by Dana. Not much, however, is gained by that; first, because we ought to regard the lines of greatest heat as well, and secondly, because it is not the degree of temperature alone, but the extent of variation which has a harmful influence. It is proved, in some instances, that the same animals can live at very different temperatures, at 3°, 7°, 15° Centigr., if only this temperature at the respective localities remains fairly constant to 3°, 7°, 15° Centigr.

These are eurythermal animals, but they are, nevertheless, stenoi (restricted), not with regard to temperature, but with regard to its variations.

I think a strange occurrence in marine life, upon which Semper laid stress, can be explained in this manner. Some Hexactinellids, typical sponges of the deep sea, occur in the Pacific Ocean, in very different temperatures; even at a comparatively small depth in tropical parts, in which the temperature is about 10° higher than in greater depths of the China Sea, in which they are to be found also. The factor which all these localities have in common, besides the absence of currents, is slight variation of temperature.

We have seen that the Nekton animals, being eurythermal, cannot be appealed to in *proof of anything* regarding the influence of

temperature; but the case is different with the two remaining biological groups of marine life, the Plankton and the Benthos.

The difference between the latter is the second point hitherto often neglected in the question of the influence of temperature.

By the term *Benthos* is meant any animal which passes its life in connection with the ground, whether it be the bottom of the deep water or the shore of the shallow water. We may distinguish, according to Haeckel, sessile Benthos, *i.e.*, animals totally fixed, and ambulatory Benthos, *i.e.*, animals capable only of creeping and crawling over rocks and sands, but not of swimming great distances at will like the Nekton.

Plankton is capable of travelling long distances, not at will, but only floating or drifting about with the currents. We might further distinguish a littoral Plankton, the Plankton of the shallow water, the shore, and the Plankton *par excellence*, that of the High Sea.

Distinct as these groups are, there may, nevertheless, be transitions between, even in the life of one single species. A Medusa, as fixed at the stem of a Polypoid, is a Benthos animal—when set free, an animal of the Plankton. But such cases occur only among the animals of the littoral Plankton, not in the Plankton of the high sea; there are other Medusæ which never have a sessile stage in their life, and are, like Salpæ, Radiolaria, etc., Plankton animals *par excellence*.

These different groups should not be confused with regard to their relations to temperature. Among the animals of the Benthos there are, as I have said, a number of eurythermal ones, as shown by experiment. But the majority are tuned, as it were, to a certain degree of temperature, which, just like the relations to the light, to the character of the bottom, and so on, must be reckoned among the peculiarities of the species. The Benthos animals are mostly stenothermal, and hence the influence of the temperature on them is very remarkable.

The coasts of the sea might, therefore, be divided, in the first instance, according to latitude or isocrymal lines, into *zones*, which are broken up into *areas* by a longitudinal division according to the continents.

Dana, Günther, and Agassiz have given us very valuable guidance for the animals of the shore. It is true that, by adding the longitudinal division to the zonary, *temperature*, which effects the arrangement into zones, does not remain the only factor in the distribution of the Benthos, as the facies of the bottom, etc., are not to be neglected. Still it is regarded as the most important one.

The distribution of the pelagic life of the open sea is not so clear. The longitudinal division is now without significance, because the influence of the continents does not come into the question. It would, however, be incorrect to think that the true Plankton animals are quite cosmopolitan.

From the *à priori* point of view, the warmth of the sun, different

in different latitudes, ought to regulate the distribution of real pelagic life; a system of belts would result parallel to the degrees of latitude; but this system of parallelism becomes disturbed by the action of currents and winds. On the other hand, by these currents sharper regions are defined; for, otherwise, all these zones would pass into each other gradually, which is not the case in reality.

Both north and south of the equator three circles of currents may be distinguished from 0–10, 10–50, 50–80° of latitude, and between them islands, as it were, of quiet water are formed, the so-called Halistases. These circles, *i.e.*, depending upon the temperature in them, constitute the most important factor in the distribution of pelagic life.

A current always shows differences of temperature at its outer limit, and by this circular arrangement the pelagic animals, as Brandt first laid stress upon, are kept within temperatures of a fairly constant degree; being always carried back to the circle-current of the temperature, and not being able to escape and flourish in the adjacent colder or much warmer water. This, as I wish to emphasise, is valid for the stenothermal animals. Eurythermal animals could be carried away to other regions, and continue to flourish there. Currents, therefore, act in two manners, either as helps to wandering, or as barriers to spreading.[2] In general, the latter will be the case for Plankton animals.

A division of the open sea into zones can, therefore, be accomplished. How great the difference can be on each side of a current may be seen chiefly at the Gulf—or Florida—Stream, as is illustrated by some instances noted by the Plankton Expedition.

While working out the Craspedota of this expedition, I could ascertain that no pelagic species of Medusa, occurring to the north of the Gulf Stream, is found also to the south of it, and *vice versâ*. The well-known *Aglantha digitalis* (described first by Forbes) has been caught constantly and in abundance from North England to Greenland. To the south of the Gulf Stream it ceases at once to occur. The same may be said of a Narcomedusa, *Solmaris multilobata*. On the other hand, *Aglaura hemistoma*, which has not been found in the north district, occurs with great regularity in the middle and equatorial part of the Atlantic, and the same can be said for the well-known *Rhopalonema velatum* and some species of *Liriope*.

Similar observations have been made in other groups of animals, for instance, in the pelagic annelids. The Tomopteridæ are abundant

[2] The occasionally abundant occurrence of animals generally rare or totally wanting at the British coast, can thus be explained. The animals in question are stenothermal animals of the Plankton. In ordinary years the Gulf Stream forms a sharp limit for them. If they are driven in the colder water by the current, they cannot grow there, but perish. Now, such uncommon occurrences have been recorded in years of uncommonly warm weather. Then the animals carried away from the south find a suitable temperature at the surface in the north, too, and the action of the current in such cases is dispersing.

in the north district, the Alciopidæ are wanting there, and occur only south of the Gulf Stream. For other groups, such as Appendicularia and Decapods hitherto worked out, the same sharp limit has been found. Most sharply defined limits are seen in the interesting pelagic copepod genus *Copilia*, which is wanting in the north district, but occurs not only abundantly but in many species in the equatorial part.

It is remarkable that, in the eastern district near Ascension, other species than those occurring in the same latitude near Brazil have been found. I have stated a similar fact for some Medusæ.

The explanation of this is rather simple in connection with the influence of temperature. Here a southern colder stream passes; stenothermal animals which are adapted to colder water find here a temperature that suits them, while they cannot grow and flourish in the tropical waters near Brazil. The influence of this same cold current has been stated to exist by Studer and by the "Challenger."

The family of Geryonid Medusæ in general, to which the genus *Liriope* belongs, are a very good example for the influence of temperature on Plankton animals, as Corals are for the Benthos. The Geryonids seem to require warm water and occur only at a certain latitude. They are abundant in the Mediterranean, but are wanting totally in the German Sea and the north district in general. They are abundant in the equatorial part of the Atlantic, and the same may be said of the Pacific. I have obtained them in great number from the warmer currents, which the "Albatross" expedition encountered. I also got them from the Red Sea and the Indian Ocean, but none from higher southern latitudes.

It is in harmony with this evident need of warmth that the Geryonidæ are typical animals of the surface. This is obvious from the so-called "Stufenfänge," *i.e.*, captures made at the same station in 50, 100, 200 m. depth; and they occurred even in the cylindrical surface-net of the Plankton expedition, where many other animals which had been found in 100 m., etc., were wanting. It is known by direct observation how their umbrellas project above the surface of the water, especially during contraction, so that Italian fishermen call them "capelletti di mare," or "little sea-caps."

From Agassiz's last three cruises there is the same result. A *Liriope* was the most abundant Medusa, always appearing at the surface, even when *Stomobrachium* and other species had retreated to a depth of 100–200 m., to avoid the influence of daylight and warm sun.

Here we arrive at the third and last point, the distinction between vertical and horizontal differences of temperature, between which a parallel has been drawn hitherto, but upon insufficient grounds. Down to the depth of 400 m. it is allowable to make a comparison between the changes in the life-conditions, especially temperature in different depths and the changes of the temperature of the surface in different latitudes; but in greater depth this becomes impossible, the light, the

atmospheric pressure becoming quite different. Even the temperature alone cannot be compared, for near the surface it diminishes very rapidly, and then more and more slowly until we reach the temperature of the abysses. Moreover, at a given time the temperature of a certain depth may be the same as that of the surface in a higher latitude, but the variations are quite different. In the Mediterranean the daily variations cease totally at 18 m., the seasonal at about 400 m., and as we have seen, the variations are much more important than the average temperature.

It was formerly concluded on theoretical grounds that there could be as well many vertical belts of different life as there are horizontal ones, but this comparison is, as I have shown, not correct or can only be carried to certain depths, not exceeding 400 m. This is not a theoretical conclusion only. All the unerring data, proving a relation between the diminishing of temperature in horizontal and vertical direction, are obtained in depths within the 200 m. limit.[3]

Considering the above facts, I do not think the horizontal distribution of the pelagic fauna is compensated by the vertical differences, so that pelagic animals would find the same temperature that they have near the surface in higher latitudes in a low latitude in greater depth. Vertical migrations cannot be so extensive (owing to the small power of locomotion of the Plankton animals); and in the greater depths conditions prevail which exist nowhere at the surface, in no latitude, and in no time of the year.

Somewhat striking are the observations of Chun, who found the surface animals of the winter in the Mediterranean at considerable depth in the summer. Setting aside the question as to whether his apparatus was adequate to prove with certainty the depth from which the animals are supposed to come, his observations can be explained by the influence of temperature.

In the Mediterranean the deep water is much warmer than in the Atlantic; the retreating animals would, therefore, not find such extremes of temperature as in the open ocean (which is, as Agassiz has stated, in some places, at least, totally destitute of pelagic life in such depths). In the Mediterranean, therefore, an intermediate pelagic fauna would be able to exist under the special life-conditions.

This is in full harmony with the investigations of the Prince of Monaco on the survival of the deep-sea animals of the Mediterranean. Formerly it was thought that the great atmospheric pressure is the most important factor in deep-sea life, and it had been concluded that the deep-sea animals coming to the surface would be killed principally by the great difference of the atmospheric pressure; but that has been refuted by the Prince. Atlantic animals

[3] *Echinocardium kurtzii* / *Moïra atropos* } occur in the Florida stream in a depth of 25 m., in North Carolina at the surface.

Aeginopsis mediterranea { in the Mediterranean near the surface, in the tropical Atlantic in 200 m.

from 2,000 m. died when they came to the surface, while Mediterranean animals from 2,000 m. survived.

The Atlantic forms came from 0° to 20°, while those of the Mediterranean came from 13° to 20°. Hence it is obvious that temperature is here the most important factor.

In so far as real pelagic life is concerned, I therefore think that the existence of an intermediate fauna is not to be regarded as proved, neither in theory nor fact, and I would like to repeat Wyville Thomson's old sentence:—"The fauna of the deep water is confined to two belts, one near the surface, the other at the ground."

According to my opinion, the abyssal fauna is chiefly a Benthos fauna, more or less in connection with the bottom; a kind of *littoral* Plankton may be associated with it, but the true pelagic fauna does not exceed a certain depth.

I do not deny there is an intermediate zone of life, but it might be supposed to consist, scanty as it is, principally of Nekton, that is to say, of swimming animals, to which vertical distances are of no importance, and which are not sensitive to the changes of temperature. All of these are eurythermal, while, in contrast to them, most of the animals of the Benthos and Plankton are stenothermal.

In conclusion, after so much *theory*, I would merely repeat the *fact* that no case illustrates the great influence of temperature on the distribution of marine animals better than the well-known example of the Corals for the Benthos animals and the new one of Geryonid Medusæ for the Plankton.

OTTO MAAS.

V.

On the Dispersal of the Nutlets in certain Labiates.[1]

ONE of the commonest methods of seed dispersal in plants with capsular fruits is what is sometimes called the "censer mechanism." The seed-vessel is open at one end, and the seeds which lie loose at the bottom are thrown out by the swaying of the plant in the wind or by the disturbance of a passing animal. A familiar instance is afforded by the larkspur *Delphinium*, where the seeds collect at the bottoms of the follicles, and are easily scattered by shaking or striking the plant. Other instances are to be found in Hildebrand's well-known book on the dispersal of seeds (1). In some of the Labiates a condition of things biologically similar exists, but here the calyx plays the part of the censer, and it is the nutlets, not the seeds, that are shaken out.

In some cases arrangements are found which seem to render the dispersal more difficult, but it has been suggested that there is here a contingent advantage to the plant, because the period during which the seeds continue to be scattered is prolonged, besides which all the seeds are not likely to be thrown out in one direction, which would be the case if they fell out easily at the first waft of wind. The most usual arrangement is that the capsule stands upright on its stalk with a small opening at its upper end. Then, again, it often happens that the seeds are not free, but adherent to the placenta, so that a somewhat violent shock is needed to displace them. In the case of the Labiates it seems possible that the hairs which line the calyx in some species play the same part.

As a further refinement come those cases in which the capsule does not remain permanently open. Steinbrinck (2), Kronfeld (4), Beck (5), and others have described cases in which the mouth of the capsule closes in wet weather and opens again when the air is dry. A biologically parallel case has been noted by Rathay (3) in certain Compositæ where the involucre bends in and encloses the achenes in moist air. A similar state of things is described lower down among the Labiates.

The special advantage gained by these adaptations is not obvious. We have no evidence that the plant profits by the seeds or fruits

[1] Read at the Oxford meeting of the British Association, 1894.

being protected from wet, which is a possible end for such an adaptation.

Lastly come those cases of somewhat rare occurrence, in which the capsule or calyx, as the case may be, opens in damp air and shuts up when dried. These movements, with which others are coordinated, form the subject of the present paper. How far they are of biological importance to the plant it is not at present possible to say. It is only by the collection of a large number of instances that we can hope to arrive at a conclusion.

Hygroscopic movements, apparently adapted for the dispersal of seeds in wet weather, are confined chiefly, although not by any means entirely, to plants indigenous to countries having long periods of drought, during which the seeds, if dispersed, would have little chance of germinating.

Familiar instances of this phenomenon are found in the Crucifer, *Anastatica hierochuntina* (Rose of Jericho) and the Composite, *Asteriscus pygmæus* (Coss et Dur), both of which are inhabitants of deserts.

In 1878 Steinbrinck (2) drew attention to the fact that the capsules of two species of Veronica open more widely when sprinkled with water, and in 1883 he gave an account (6) of a similar movement in the capsules of *Caltha palustris* and several species of Mesembrianthemum and of Veronica. This was found by MacLeod (7) to hold true of *Veronica arvensis* and *V. serpyllifolia*. Volkens (8) and Schinz (9) have described other cases of hygroscopic movements of a like character.

Verschaffelt (10) has described the striking instances of *Salvia horminum*, *Brunella vulgaris*, *B. grandiflora*, and *Iberis umbellatum*. Finally, Ascherson (11) has given to these movements the name of hygrochasia; and, after reviewing critically the literature on the subject, he describes two new cases, *Lepidium spinosum* and *Ammi Visnaga*.

On examining about forty Labiates in fruit I have found fourteen species which show hygroscopic movements. Of these there are eleven species which move in such a way as apparently to favour the dispersal of their nuts in wet weather.

The method of observation was in each case as follows. The plant in the dry condition was drawn to scale. Water was then sprinkled on it, and when the consequent movements had taken place, a drawing was again made. In order to observe the more minute movements of the calyx-teeth, a single calyx was pinned down to a cork, under a microscope furnished with an eye-piece micrometer, and the change in the position of the teeth noted, when the calyx was wetted.

The movements may be classified in the following way:—

I.—The movements closely resemble those of *Brunella vulgaris* described by Verschaffelt (10).

The axis of the calyx is turned upwards so as to be almost vertical (the mouth being up), and the opening is more or less com-

pletely closed by the calyx-teeth. In rain the pedicels bend outwards until the axes of the calyces are nearly horizontal. At the same time the calyx-teeth bend out so that the mouth is open.

1. *Ziziphora capitata* (Linn.), Figs. 1–4.

A herbaceous annual, with erect stem. The flowers are borne in verticillasters, forming a dense terminal head. When in fruit, the four leaves immediately beneath the head are folded up round it, so as almost to enclose it. The calyces are almost erect, and the mouth quite closed by the teeth. The calyx is tubular, elongated, hairy outside, glabrous within, except for a thick ring of hairs at the throat. Rain or moisture cause a rapid movement to take place. The leaves folded up round the head bend out rapidly; the calyces bend outwards, so that they become more horizontal, and the calyx-teeth open. By sharply shaking the plant the nuts are thrown out to some distance.

EXPLANATION OF FIGURES.

FIG. 1. *Ziziphora capitata*; a capitulum in fruit; in the dry condition × 1¼ c.ca.)

FIG. 2. The same; wet condition.

FIG. 3. Calyx of *Z. capitata*; dry condition.

FIG. 4. The same; wet condition.

FIG. 5. *Ziziphora tenuior*, a spike in fruit; dry condition. (× 1.)

FIG. 6. The same; wet condition.

FIG. 7. A small portion of the spike of *Z. tenuior*, most of the leaves and calyces having been removed; dry condition.

FIG. 8. The same; wet condition.

FIG. 9. The mouth of the calyx of *Z. tenuior* in the dry condition; longitudinal section, showing the ring of hairs in the throat and the teeth closed.

FIG. 10. The same; wet condition.

FIG. 11. *Lallemantia peltata*; a node of the spike, all the calyces and bracts having been removed except two; dry condition. (× 1½.)

FIG. 12. The same; wet condition.

FIG. 13. Mouth of the calyx of *L. peltata*, seen from the front; dry condition, the teeth closing the opening.

FIG. 14. The same; wet condition; the teeth having moved slightly apart.

FIG. 15. *Ocimum campechianum* (?); part of the spike in fruit; dry condition. (× 1½.)

FIG. 16. The same; wet condition.

FIG. 17. A calyx of *O. campechianum*; dry condition; the others in same whorl having been removed.

FIG. 18. The same; wet condition.

FIG. 19. The same; longitudinal section, showing the hairs at the throat.

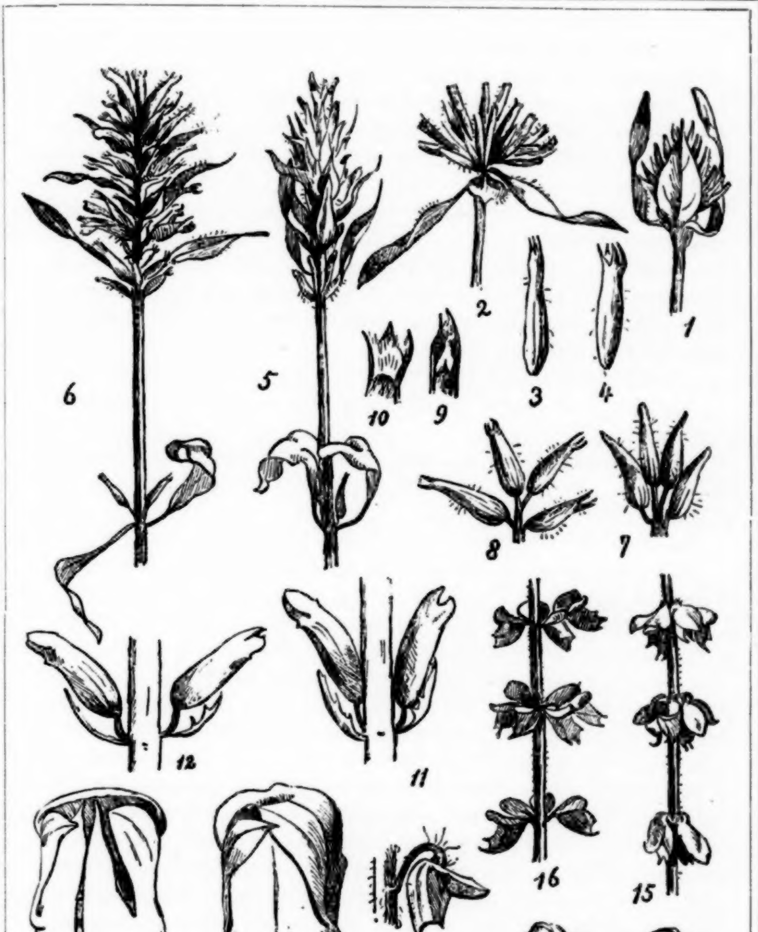
FIG. 20. Part of the spike of *Salvia gigantea*. (× 1.)

FIG. 21. A calyx of *S. gigantea*; dry condition.

FIG. 22. The same; wet condition.

FIG. 23. *Lamium flexuosum*. A calyx seen from the front, showing the teeth curved across the opening; dry condition. (× 5.)

FIG. 24. The same; the teeth bent apart; wet condition.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Habitat: Region of the Mediterranean and South Russia—fields and hills.

2. *Ziziphora tenuior* (Linn.), Figs. 5–10.

A herbaceous annual. The flowers are in few-flowered axillary verticillasters, forming a spike. The calyx is like that of *Z. capitata*, but more hairy externally. The movement is similar to that of *Z. capitata*. Both the floral leaves and calyces, which when dry are turned up round the main axis, bend out when wet away from it, and the calyx-teeth open.

Habitat: Region of the Mediterranean and South Russia—hills and uncultivated fields.

3. *Ziziphora hispanica* (Linn.). (Only a herbarium specimen of this was examined.) The stem is branching and herbaceous; the flowers grow in dense spikes. The movement is the same as in *Ziziphora capitata*.

Habitat: Spain.

4. *Ziziphora taurica* (Bieb. Fl. Taur. Cauc.). (Herbarium specimen.)

The flowers are in long leafy spikes; the movement the same as *Z. capitata*.

Habitat: Mount Taurus.

5. *Brunella hyssopifolia* (Lam. fl. fr.).

The movement is the same as that of *B. vulgaris* described by Verschaffelt.

Habitat: Region of Mediterranean Europe—open, dry places.

6. *Lallemantia peltata* (Fisch. et Mey.), Figs. 11–14.

An erect herb, with six-flowered verticillasters in the axil of sessile bracts. In fruit, the tubular calyces are nearly erect, and the bracts subtending them are folded up so as partially to enclose them. The pedicels of the calyces are flat and broad, being compressed in the median plane. The teeth are unequal, the posterior one being much the largest, flat and slightly curved forward over the four anterior teeth which almost close the mouth. In consequence of wet, the bracts and calyces bend down and outwards from the main stem until the axis of the calyx is at about an angle of 45 degs. At the same time, the four anterior calyx-teeth move slightly apart so that the opening is enlarged and the nuts are shaken out much more easily than when dry.

Habitat: Persia, Armenia, near the Euphrates, Kurdistan, Mount Elburz.

II.—The axis of the calyx points more or less downwards, and in consequence of wet it bends up until nearly horizontal. *Salvia horminum*, described by Verschaffelt, is the most marked example of this.

1. *Ocimum* [probably *campechianum*] (Mill), Figs. 15–19.

The stem is erect; the flowers are arranged in six-flowered verticillasters. The calyx when in fruit is bent downwards. The posterior tooth is broad and obtuse, with winged and decurrent

margins ; the other teeth are ovate ; the two lateral ones very shortly pointed, the two anterior ones more pointed. The mouth is open, but a ring of hairs closes the throat.

In consequence of wet the pedicel bends up so that the calyx becomes horizontal ; the two anterior teeth bend slightly down, and the two lateral teeth move a little apart, so that the opening of the mouth is slightly enlarged. The movements of the teeth can only be seen under the microscope.

Habitat : Central America.

2. *Melissa officinalis* (Linn.).

The stem is erect; the flowers are in lax, few-flowered axillary verticillasters. The calyx is pedicellate, two-lipped, slightly inflated, the mouth open. The upper lip is spreading, truncate, shortly three-toothed ; the lower lip is bifid, the two pointed teeth either pressed against each other or crossing each other. The throat is lined with a ring of hairs, which closes the mouth. The whole calyx is rather deflexed.

In consequence of wet the pedicels straighten, so that the calyces are raised to a nearly horizontal position. The anterior teeth are also lowered very slightly.

Habitat : S. Europe and M. Asia—woody hills.

3. *Salvia sclarea* (Linn.).

The flowers are arranged in six-flowered verticillasters. The calyx is shortly pedicellate, hairy, slightly bent downwards. The upper lip is tridentate, the lower bifid with acuminate teeth. In consequence of wet, the pedicels bend slightly up, so that the calyx is raised, and at the same time the teeth of the lower lip are very slightly lowered.

Habitat : S. Europe,—roads and dry places.

4. *Salvia gigantea* (virgata, ait. hort. kew.), Figs. 20–22.

The flowers are arranged in six-flowered verticillasters. The calyx is shortly pedicellate, campanulate, hairy. The teeth of the lower lip bend up so as nearly to close the mouth. The movement in consequence of wet is the same as in *Salvia sclarea*, but the downward movement of the lower lip is more marked.

Habitat : Italy.

III.—The movement is confined to the calyx-teeth.

Lamium flexuosum (Ten. fl. nap.), Figs. 23, 4.

The stem is erect ; the flowers are in dense axillary verticillasters. The calyx is bent downwards below the horizontal, subcampanulate, lined with short hairs. The mouth is partially closed by the teeth, the posterior tooth bending down forwards, and the two lateral ones bending across, so as to meet.

In consequence of wet the posterior tooth bends up until it is horizontal, and the lateral teeth bend out sideways, so that the nuts can be shaken out more easily.

Habitat : Liguria.

Besides the eleven species described above, there are three which also move hygroscopically, but in a different way.

These three, *Hyptis pectinata*, *Elsholtzia cristata*, and *Nepeta kohamerica*, have the axis of the calyx almost erect or else horizontal, and the mouth not closed except by hairs, and in consequence of wet the teeth close slightly. This movement is probably not connected with the dispersal of seeds, but with their protection from wet.

1. *Hyptis pectinata* (Poit. ann. Mus.).

The calyx is almost regular, campanulate, the mouth open, with spreading pointed teeth.

The flowers are arranged in cymes, which, when in fruit, are elongated, unilateral and pectinate, with the calyx turned vertically upwards.

In wet the peduncles bend up so as to bring the calyces near the main stem, and the calyx-teeth bend up so as to close the mouth.

Habitat: Equinoctial America—dry chalky and sandy places. Mexico, Tropical Africa, etc.

2. *Elsholtzia cristata* (Willd).

The flowers are in many-flowered verticillasters forming dense unilateral spikes. The calyx, in fruit, is regular, campanulate, the exterior hairy and glandular, the interior glabrous except for long hairs on the margin of the teeth which almost close the mouth. The axis of the calyx is horizontal or inclined upwards.

In consequence of wet, the calyx-teeth close together partially.

Habitat: Asia, and Europe introduced.

3. *Nepeta kohamerica* (Regel).

Stem erect branched. Flowers in terminal spikes. The calyx is hairy and tubular, with sub-oblique mouth, the calyx-teeth lanceolate. In consequence of wet, the calyx-teeth close slightly together.

Habitat: Plains of Kohamyr.

With the exception of the three last cases, the movements described above appear to favour the dispersal of the nuts in rain, yet it is not easy in every case to demonstrate this fact. In *Lallemantia*, the nuts are far more easily shaken out in the wet condition, but in *Ziziphora tenuior*, which has similar movements, this was not found to be the case. Possibly this may be due to the nuts not being ripe.

Salvia sclarea and *Melissa officinalis* have been placed with the others, but their movements are so slight that it is difficult to see how they can affect the dispersal of the nuts. In *Ocimum*, too, although the movement is very decided, yet the advantage gained is not evident.

In conclusion, I should like to express my thanks to Mr. F. Darwin, for having suggested that I should examine Labiates in regard ot the distribution of their seed, and also for his very kind help and criticism.

REFERENCES.

1. **Hildebrand.**—" Verbreitungsmittel der Samen."
2. **Steinbrinck, C.**—" Untersuchungen über das Aufspringen einiger trockenen Pericarpien." *Bot. Zeitung*, xxxvi., 1878.
3. **Rathay.**—" Austrochnungs und Imbibitionserscheinungen bei Cynareen Involucren." *Sitzb. d. k. Akad. Wiss.* Wien, 1881.
4. **Kronfeld.**—" Verbreitungsmittel d. Compositenfrüchte." *Sitzb. d. k. Akad. Wiss.* Wien, 1885.
5. **Beck, G.**—" Untersuchungen über den Oeffnungsmechanismus der Porenkapseln." *Verhandl. Zool. Bot. Ges.* Wien, 1885.
6. **Steinbrinck, C.**—" Ueber einige Fruchtgehäuse die ihre Samen infolge von Benetzung freilegen." *Ber. d. deut. bot. Ges.* Berlin, 1883.
7. **MacLeod, J.**—" Veronica arvensis und V. serpyllifolia." *Bot. Jaarboek*, 1887.
8. **Volkens.**—" Die Flora d. ægypt. arab. Wüste," 1887.
9. **Schinz.**—*Abh. Bot. Ver. Brandenb.*, xxxi., 1889, p. 186.
10. **Verschaffelt.**—1. " Die Verbr. d. Samen bei *Brunella vulgaris*, *B. grandiflora*, *Salvia horminum*, *S lanceolata*." *Bot. Jaarboek*, 1890.
    2. " Die Verbr. d. Samen *Iberis amara* u. *I. umbellatum*." *Bot. Jaarboek*, 1891.
11. **Ascherson, P.**—" Hygrochasie und zwei neue Fälle dieser Erscheinung." *Ber. d. deutsch. bot. Gesellsh.*, x., 1892.

DOROTHEA F. M. PERTZ.

VI.

Hertwig's "Preformation or New Formation."[1]

Part III.

DR. HERTWIG entitles the concluding part of his paper, "Thoughts Tending to a Theory of Development of Organisms." He professes, not to work out a theory in detail, but to indicate the direction in which guidance is to be obtained. Let it be remembered that in his criticism of Weismann's preformationist views there were two main points. First, he thinks not only that Weismann has failed to prove the existence of "heirs-unequal division," but that there are many grounds for supposing that all cell-division is a case of "heirs-equal" division. Hence there is no reason to suppose that the various elements of the germ-plasm are marshalled to their places in the course of an individual development, and that this sorting out of preformed determinants is the reason why an egg produces with imperative fidelity the thousand different cells of the adult. Every cell of the whole organism, so far as inherited dower goes, is identical. Next, Hertwig throws doubt upon the whole conception of determinants, pointing out that very many of the individual characters of organs and of organisms are not properties of cells at all, but are properties of aggregates of cells, and therefore cannot come into existence until aggregates arise, and certainly can have no preformed determinant within a single cell.

Hertwig is disposed to steer a middle course between preformation and epigenesis, to cull from either theory what seems reasonable and in consonance with observed fact, and to reject much of the speculation with which both preformationists and epigenesists have rounded off their conception of Nature. As yet, we are far from having a complete explanation of the problems, but it seems to him as if Nature were a follower neither of Weismann nor of his opponents, but evenly pursued a middle way.

It is necessary to suppose that each organism starts with a specific dower. However much can be attributed at present, or may come to be attributed, to the moulding forces of environment, there

[1] Zeit- und Streitfragen der Biologie. By Professor Dr. Oscar Hertwig. Pamphlet I. Präeformation oder Epigenese? Grundzüge einer Entwicklungstheorie der Organismen. Pp. 144, with 4 illustrations in the text. Jena: Gustav Fischer, 1894. Price 3 marks.

remains a large residuum, which can be explained only by the transmission of a specific plasm from the parent to the sexual cell. In the same tank of sea-water, surrounded by the same baneful and useful circumstances, there may lie two fertilised egg-cells not differing appreciably in appearance, in size, in chemical composition, or in physical characters. Yet one of these produces a sea-hedgehog, another a worm. The resemblances between the eggs of species of the same genus may be still more remarkable, and the distinctive characters may not appear till a late period in the course of the individual development; but the different specific characters do appear, although there is nothing in the environment to cause them. The egg of a barn-door fowl is as fully stamped with the character of its species as is the adult animal.

To explain the process of development, Hertwig believes that we must postulate the existence of a material which contains germs or incipia of a particulate character, and which possesses an extraordinarily complicated structure corresponding to the marvellously minute and complicated different ways in which it must react, as the growing organism is built up by cell-multiplication. This material differs from Weismann's conception of germ-plasm in many ways. First, it is not built up of determinants and so forth into a complicated architecture. Hertwig makes no supposition whatever as to the physical nature of his specific material. Next, it by no means contains all the determining causes of the organism. Many of these are due to external causes: many more to the reaction between external causes and the material itself. In this respect, it is true, there is no vital difference between the two theories. Weismann at first attributed too much to the germ-plasm and overlooked a number of ways in which the environment plays a direct part in ontogeny. But recently he has reviewed and accepted many external causes which he had hitherto passed by. Both Weismann and Hertwig regard this specific material as containing the residual factors of development. Weismann may understate, or perhaps Hertwig overstate, the forces which have to be allowed for before the residuum is reached; but both are agreed that there is a necessary residuum for which a specific inherited material must be assumed. We come now, however, to the great point in which the two views are wide as the poles asunder. Weismann, as everybody knows, attributes to the sexual cells, and to special regions called germ-tracks, the exclusive possession of the germinal plasm, making an exception only of a few cases of special and probably secondary adaptations. In consequence, he draws a sharp distinction between the cells which contain the plasm and the somatic cells. New organisms arise only from cells containing germ-plasm, and the somatic cells, when they become specialised, lose the power of reproducing the species as they contain, not germ-plasm, but a simpler derivative of it. Hertwig, on the other hand, totally denies this sharp distinction. All the cells in the body receive an equal dower of the specific

material, because in cell-division there is always an equal division. No doubt sexual cells, for the most part, arise only in special parts of the organism. But, in these cases, the fact that certain regions of the body give rise to the sexual organs is merely a part of the general ordering of the body into tissues and organs, and is not due to the presence in sexual cells of any special plasm. Hence it is not surprising to find in special cases diffused powers of reproduction or of regeneration, any more than it is surprising to find muscle-cells or gland-cells in unwonted situations.

In one matter, the possibility of the inheritance of acquired characters, a question which bulks more largely in popular controversy than in Weismann's theory, the two views do not differ theoretically. Weismann admits the possibility of the influence of outside forces modifying the germ-plasm in the cells, and as Hertwig's specific material is diffused through all the cells of the body, it too is not removed from the possibility of being altered by the environment. Hertwig, however, does not specially deal with the controversy as it is known, but one draws the inference from the general tendency of his views, that his specific material is supposed to have as much organic stability as the germ-plasm.

The epigenetic part of Hertwig's views about development consists of an account of many of the factors, apart from a specific plasm, which direct the development. The leading motive in his account of these is one diametrically opposed to Weismann's view that a cell must become what it is, and develops only in accordance with the determinants which it received from among the determinants in the parent cell from which it arose. Hertwig believes that a cell becomes whatever it is made to become by the forces round about it. The limit to this is given by the specific material with which it is endowed. A cell of one animal could not be made into the cell of another, but the cell of an animal might have been any other cell of the animal had its environment been different. In the words of Driesch, the specialisation of a cell is a "function of the locality of the cell in the organism." The causes of the elaboration of an organism from its germs or incipia lie in conditions outside the incipia in the egg-cell, but which, nevertheless, follow in regular and orderly sequence. The first of these causes is the continual change in the relations of cells to each other and to the external world as the organism is built up by cell-multiplication. Physiologically speaking, the differentiation of cells in different directions is due to the reaction of the organic substance to different external stimuli. The capacity of the cell to assimilate or to go through metabolic changes in the case of the fertilised egg-cell produces the cleavages by which the morula is formed. The particles of the protoplasm were arranged round the nucleus of the egg-cell as a single centre of forces. In the morula they are arranged round a number of new centres corresponding to the new nuclei. Thus, even in equal-heirs

division, differences arise in the cells according to their position and relations to each other, and the cells are different qualitatively from the original cell from which they sprang.

In many respects the form assumed by the cell-masses is a function of organic growth. The original cell, from the physical nature of the protoplasm, naturally has a spherical shape, and the retaining of this shape by the cell-mass produces differences in the shapes and surfaces of the constituent cells. The next important factor is the necessity that growth shall take place so as to secure the greatest possible extension of surface in proportion to mass. A crystal grows by the deposition of new particles upon the outside; organic matter grows by the intersusception of matter from the different constituents in the environment, and it is therefore necessary that as large a surface as possible shall be exposed to the environment in order that the varied substances may be picked up.

The organic material in process of growth can assume only those forms which allow it to remain in touch with the outer world. A cell-mass could not grow indefinitely thick without the inner cells being deprived by the outer cells of their relations to the environment. For this reason, cell-multiplication produces threads and layers and membranes, rather than solid masses. In the plant kingdom the cells, for the most part, are strengthened by a firm cell-wall, through which their liquid and gaseous food is able to pass. The possession of this cell-wall enables plant growth to remain, for the most part, in the form of threads and membranes, and the necessary extension of surface is produced in the simplest possible way. In the adult plant all the organs and tissues grow out so as to preserve this simple extension of surface. In the animal kingdom the method of nutrition by the absorption of solid masses into the interior of the cells prevents the formation of thick cell-walls. Animal growth, then, cannot rely upon the nature of the individual cells to secure the necessary rigidity and coherence of the whole mass. The cell-masses grow so as to form hollow bladders and tubes, the whole structures possessing a rigidity not found in the individual cells, and growth takes place by the infolding of the single layers of cells which form the walls of the spheres and tubes. In plant-tissues the food-supply comes from the water and the air of the outer world, and in consequence the extension of surface is as much as possible an extension turned to face the outer world. In animals the solid food is taken into an internal cavity, where it is broken up and ingested, and in consequence there is as great as possible an extension of this inner surface. Thus, the blastula is a hollow sphere, the walls of which consist of a single layer of cells which retain contact with the environment on their inner and outer surfaces. An inpushing of the wall of the blastula gives rise to the gastrula, and subsequent infoldings in the inner and outer walls of that give rise to muscle layers, nerve layers, cœlomic pouches, and so forth. But in all these

changes the cells in different places find themselves in very different circumstances; those lining tubes and hollows inside the body are brought in contact with very different external forces from those lining the outer side of the body. In correspondence with these varied stimuli the organic material, the same in all the cells, responds in different ways, producing cell-specialisation.

In the epigenetic modification the next process which helps to produce the specialisation of the organic body is of a physiological nature. Every cell is an elementary organism, endowed with the capacities of elementary organisms. But the cells resulting from the division of the egg to a certain extent lose their individuality in the new individuality of the whole. The purely morphological method of regarding cells tends to exaggerate their individual importance. Although the whole shape and form of the organism is produced by cell-multiplication, yet, considered as an organism, the multicellular adult is better regarded as a continuous mass of an organic material. Hertwig details many of the facts showing the intimate correlations which exist between different parts of organisms, and concludes that many of the elaborations of development must be set down to the observed fact of correlative growth. This part of his argument is perhaps more conclusive in so far as it is a criticism of Weismann's doctrine of the existence of associated determinant to account for correlations, than it is in itself satisfactory. Weismann may have failed completely in giving a plausible explanation, or he may have underrated the importance of correlation as a factor in growth. Hertwig simply associates the two observed facts of the physiological unity of organisms and the existence of correlated growths.

He elaborates at some length his view that different external influences cause the development of identical incipia into different final results. In course of this, he reviews the controversy between Spencer and Weismann relating to the polymorphism of social insects, and concludes with Spencer against Weismann that Natural Selection is not the factor at work, but that the different environments of the different forms have produced the divergent modifications. He sees in it the same phenomenon as the production of roots from stems, or of the upper part of a hydroid from the cut lower part. It is one of the results of the identity of the organic matter all through the cells and tissues of an individual, and of the capacity of this identical matter to respond in varied ways to varied stimuli.

The three great points of his theory are these. First, all organisms possess a specific plasm of exceedingly complicated nature. Secondly, the sexual cell, which is the origin of a new organism, possesses a share of this plasm, and, being an elementary organism, is able to grow and multiply. In this process of multiplication, the resulting cells, formed as they are by equal-heired division, receive each an unmodified share of the initial plasm, which, like the cells

containing it, increases in bulk by growth. Thirdly, the specialisation and differentiation of the organs and tissues is due to the action of varied external stimuli upon the same organic substance. In his own words :—

"My theory may be called *evolutionistic*, because it assumes the existence of a specific and highly-organised incipial plasm as the basis of the process of development. It may be called *epigenetic*, because the incipia grow and elaborate from stage to stage only in the presence of numerous external conditions and stimuli beginning with the metabolic processes preceding the first cleavage of the egg-cell, until the final product of the development is as different from the first incipia as adult animals and plants are from their constituent cells."

P. CHALMERS MITCHELL.

SOME NEW BOOKS.

A Living "Palæontology."

Éléments de Paléontologie. By Félix Bernard. Seconde Partie. 8vo. Pp. 529–1168 and i.–viii. Paris: Baillère & Sons, 1895 (August, 1894). Price 15 francs; price of the complete work 25 francs.

We expressed our appreciation of the first part of this book in April of last year (Natural Science, vol. ii., pp. 307–308), and after carefully examining this second part, which deals with Lamellibranchs, Cephalopods, Vertebrates, and Plants, we do not feel called upon to modify the opinions previously given. In many respects this text-book of palæontology is superior to any other with which we are acquainted, and we should hesitate to say that it is in any respect inferior to those others. Its virtues are its own; its vices are common to all similar works. It is, therefore, more useful as well as more pleasant to praise the former than it is to point out the latter.

Dr. Bernard's chief merit is that he treats extinct animals from the standpoint of the biologist, laying stress on such as are of morphological importance while passing lightly over those genera and species that have acquired a fictitious value as the tokens or date-counters of the stratigraphical geologist. Evolutionist as well as biologist, he bases his classification in each group on its ascertained phylogeny, and remembers that in this difficult study evidence must be collected from the palæontologist, anatomist, and embryologist alike. Thus every fact that finds its place in this book is of a definite value, and is quoted for a definite purpose. Joined by the thread of scientific theory, such facts are easily retained in the memory of the student, and the work, instead of being a dry cram-book or catalogue, assumes the form of an interesting narrative, where the deepest truth proves the highest art. Since we can never have the conclusion of the lamented Neumayr's "Die Stämme des Thierreiches," we are grateful to Dr. Bernard for so soon applying the methods of that distinguished writer to a working text-book for students.

Turning to the execution of this volume, it is plain that Dr. Bernard has taken much trouble to get up the recent literature, and has spared no pains to discuss all views of any importance. Thus, over two pages are devoted to an account of the Ammonite aptychus and of the various explanations of it, while the different theories as to the origin of the Horse are admirably summarised. The errors that mark the work of a compiler, as distinct from that of an original investigator, are, however, easily to be found. To ascribe to Smith Woodward the views quoted on p. 800 as to the crocodilian skull does no great harm; it merely shows that the author has obtained his knowledge of fossil crocodiles from Mr. Woodward's semi-popular summary rather than from the original

sources. Similarly, though Dr. Bernard makes a point of ascribing his figures only to their original authors, we note some curiously erroneous ascriptions; for instance, fig. 402 is not by Lydekker but by David Page, while Nicholson should hardly be credited with the well-known figure of the teeth of *Cochliodus* by Agassiz. To take one more instance, no man that had ever examined a *Sepia* shell for himself could have called the "pad" the homologue of the pro-ostracum of the Belemnite, or could have denied that the posterior end was septate. Lapses such as these will, however, always and inevitably be found in the work of those writers who, in Transatlantic phrase, bite more than they can chew. The critic is obliged to point them out, and when they rise above a certain percentage he is obliged to be severe, though some offending authors are of the contrary opinion; and possibly the authors are not wholly wrong in their protest, for after all the people that really deserve chastisement are those who encourage young and untried writers to grapple with a subject that demands the co-operation of numerous specialists. The authors are whipping-boys for these, the true offenders, so let the strokes be lightly laid on!

To detail the treatment of the various groups dealt with in this second section would be lengthy and wearisome. We shall only venture on a few remarks.

The account of the Cephalopoda is clearly influenced by Munier-Chalmas, a fact by no means to be regretted, for that naturalist, though he has undoubtedly thought much, has published little on this fascinating subject. Hence we find the order Ammonoidea removed from the Nautiloidea, and boldly placed alongside of the Decapoda and Octopoda. We admit that very much may be said in favour of this association; but it is possible to go too far. A short time ago Mr. H. Woods informed the world that the Ammonoidea possessed "two pairs of gills, two pairs of auricles, and two pairs of kidneys." Now Dr. Bernard states that they are "pourvus de 2 branchies et de 2 oreillettes," and that they have a "Système nerveux à ganglions bien délimités." It is clear that these gentlemen cannot both be right; and, as neither of them has as yet offered an atom of proof in support of his assertions, it is perfectly open to us to maintain that neither of them is right. Indeed, Dr. Bernard admits that if we knew the animals that inhabited the Ammonite shells we should probably attach less importance to names derived from the number of gills. Why continue to assume a knowledge though you have it not? Owen's classification was excellent and the obvious one, so long as attention was fixed on recent Cephalopoda; but the researches of Branco, Hyatt, Munier-Chalmas and others on extinct Cephalopoda have made its retention, even in a modified form, a conspicuous absurdity.

To the families that constituted the Dibranchiata of Owen the name "Belemnoïdes" is here applied, a name that has always struck us as singularly unhappy when thus extended to Cephalopods unprovided with a belemnon or guard and even in some cases without a shell. The account of this order is well up-to-date, but we regret still to find statements that we believe incapable of proof. *Spirula*, for instance, is surely not the "Belemnoid" most nearly allied to the Nautiloid and Ammonoid type. It is coiled in the inverse direction, while the statement that its body is for a considerable period contained within the shell is a pure assumption. It is probably descended, as Dr. Bernard admits, from *Spirulirostra*, and is therefore separated from the coiled Ammonite shell by all its

ancestors at least as far back as the Trias. We believe that the Belemnoteuthidæ form the starting point of the Chondrophora rather than of the Phragmophora or Osteophora; but the question admits of argument. Neither can we agree with Dr. Bernard when he states that without doubt the Tetrabranchs have given birth to the Dibranchs, or when he maintains that some Goniatites are directly derived from some coiled Nautiloids, or when he writes that the Chondrophora are manifestly descended from Belopteridæ and Sepiadæ.

In the pages allotted to Vertebrata, the author hardly acts up to his principles in devoting so small a space to the Amphibia or "Batraciens." The extinct Stegocephali possess a peculiar interest as vying with the Theromorpha for the honour of Mammalian ancestry. Dr. Bernard indeed inclines to derive Mammals from the Theriodont Reptiles, but there is much to be said in favour of their direct descent from Amphibia. It is not so much the peculiar skeletal characters, such as the two occipital condyles, the single temporal arcade, the bony symphysis of ischium and pubis, that are in the way of their descent from Reptiles, for these characters were present in the lowest Reptiles as well as in the highest Stegocephali. But it certainly seems easier to derive the Mammalia from the ancient Amphibia with primitive vertebral column, anteriorly directed ilium, five-fingered hand and glandular skin, than it does to derive them from any such forms as had advanced in the direction of the Reptilian type. At any rate, these are questions not yet settled, and for a more complete understanding of them the student desires a better account of the Amphibia than is given by Dr. Bernard, as well as more reference to the writings of E. D. Cope.

Passing over the remaining chapters on Vertebrata, we need merely mention that the dentition of the Mammalia is very clearly described, with figures and diagrams explanatory of the numerous terms now employed. This part concludes with a fuller account of ancient man than is usually found in works of this class and size.

Having devoted 800 pages to the animal kingdom, our author proceeds to compress the whole of Vegetable Palæontology into 87 pages, which 87 pages we have little hesitation in saying are simply wasted. No one that wishes to study the subject is likely to come here for his information, while the zoologist will only grumble at the additional size of the book. The classification employed in this part would have been the better for a little revision. The essay on the phylogeny of plants pays scant attention to the views of anyone but the Marquis de Saporta. No mention is made of Treub's work on *Casuarina*, or more recent work in Russia and in this country on some of the Cupiliferæ, all of which has a very important bearing on phylogeny. A mistaken sense of duty, probably on the part of the publishers, was no doubt the cause of the insertion of this inexact and insufficient chapter.

The book closes with a few additional notes, some errata, and an excellent index. We note that the author did us the honour to read our former remarks, though he does not seem to have fully understood them.

Till better methods enable a more accurate text-book of palæontology to be compiled, we recommend this to the notice of students. It is always interesting, often original, yet judicial, well illustrated, and on the whole very good value for a sovereign.

F. A. B.

THE PHYSICAL GEOLOGY AND GEOGRAPHY OF GREAT BRITAIN. By the late Sir Andrew C. Ramsay, LL.D., F.R.S. Sixth Edition, edited by Horace B. Woodward, F.G.S., of the Geological Survey. London: Edward Stanford, 1894.

A SCIENTIFIC book which goes through five editions in fifteen years has, we may be sure, either something very attractive about it, or contains a store of information for which there is a steady or growing demand, or has the good luck to combine both claims to popularity. That the last was the case with the late Sir A. Ramsay's "Physical Geology and Geography of Great Britain," all those who know the book, and this includes everyone who is interested in its subject, will be prompt to admit. It is not merely that it gave a summary, condensed but singularly clear, of the bare facts, stratigraphical and palæontological, connected with the geology of our islands; there are many who possess the knowledge that would enable them to do this perhaps equally well. But these dry bones were made to live; clothed, as it were, with flesh and muscle and organised tissue, when it was shown how intimate was the connection between them and the scenery, the climate, the industry, and the racial peculiarities of our native land. A further charm lay in the enunciation of sundry theoretical views, due largely or wholly to the brilliant ingenuity of the author, many of which hold their own, or have even been strengthened by the advance of knowledge, while some are still matters of dispute. And all was done in such a way that the book was a vivid reflection of the individuality of the author. Those who had the good fortune to know him intimately, had brought back to them the genial talk which was the echo of his strong convictions, and the enthusiastic emphasis, tinged and heightened by a steady glow of humour, with which he insisted on the doctrines that he held to so tenaciously. Time has weakened but little, for the writer of these words, the remembrance of those "dies noctesque deorum"; and for him, and many another, no lapse of time can weaken the admiration and love for the master that grew out of them.

But other things are more transitory. Editions become exhausted, to the profit, we may hope, both of publisher and author. The growth of knowledge demands, in the new edition, corrections and additions, maybe omissions, to bring it up to date. With the majority of books the revision presents no great difficulty, and requires no more than sound judgment and an acquaintance with the latest phase of the science they treat of. In the case of such a book as that now before us, there must be besides the sympathy which enables the reviser to feel wherein lies its individuality, and the tact which will put right what is in obvious need of modification without impairing the special flavour of the work.

It is gratifying to find that these points have been kept in view by the editor of the sixth edition. Without attempting a minute collation of it, and the edition that preceded it, we are convinced that every attempt has been made to place the book on the level of the knowledge of to-day. A sketch is given of the various views held as to the British pre-Cambrian rocks. *Olenellus* is figured; the discovery of *Conocoryphe viola* is recorded; notices of Overthrust faults, of Mr. G. Barrow's paper on Themo-metamorphism, and of the occurrence of Radiolarian Chert in the Glenkin Shales and Mullien Island are inserted. The researches of Mr. H. T. Brown on the Permians of Leicestershire, and of Mr. Clement Reid on the Cromer

Section, receive due recognition. These examples, picked at random, are types of what has been done to keep pace with the march of geological research.

Without wishing to be over critical, one of his points may be noticed where there is still perhaps room for improvement. It is hard to see why, in the table on page 400, the Devonian should be sandwiched in between the Upper and Lower Old Red Sandstone. In putting forward the conjecture that stratified rocks may possibly be liquefied by intense metamorphism (p. 36), a word of caution might have been usefully added. Surely Dura Den is not the locality for *Anodonta jukesii* and *Palæopteris* (p. 94); nor can there be any doubt now as to the age of the Reptiliferous Sandstone of Elgin (p. 128). The episodal character of the Corallian Beds is hardly brought out with sufficient distinctness; and in the question of nomenclature between Neocomian and Lower Cretaceous there is something like a halting between two opinions; Glauconitic would have been better than Chloritic Marl. In the case of the Upper and Middle Eocene, the time has come to make their representatives in Hampshire and the Isle of Wight, about which our knowledge is fairly complete, the type, in place of the Bagshots of the London Basin, about which we know (*pace* Dr. Irving) scarcely anything. In the case of the Oligocene, more might have been said about the great earth movements which culminated towards the end of that epoch. Some objection may be taken to what is said about the shell-bearing gravels of Macclesfield. It is hardly possible for anyone who has seen these beds in section and collected their fossils to believe they can have been driven up by an ice-sheet from an adjoining sea bottom. The perfect state of many of the shells may be accounted for by supposing that they were frozen into the matrix during their journey. But anyone who has noted the bedding and current-bedding of the gravel can hardly fail to be convinced that they are subaqueous deposits laid down where we now find them. That the shells indicate different depths of water is natural, for the gravels were probably formed during subsidence and an increasing depth of the sea.

The later chapters, though judiciously modified, are happily allowed to retain much that gave to the book from the first its distinctive character. Lastly, by omissions, none of any serious moment, and rearrangements, often resulting in manifest improvement, the book has been reduced from 639 to 421 pages.

A. H. GREEN.

THE ROYAL NATURAL HISTORY. Edited by Richard Lydekker, B.A., F.R.S. London: Frederick Warne & Co., 1894. In monthly parts. Each, price one shilling nett.

TEN numbers of this interesting popular Natural History have now been published, and the public, Mr. Lydekker, and Messrs. Warne and Company must be congratulated. The get-up of the volume is all one could wish for, and the illustrations, of which some are taken from Brehm's well-known "Thierleben," while others are specially prepared for this work, are a great improvement on anything that has been published before. The coloured pictures, on the whole, we do not care for, though some of them are well done. The first two parts of the work are devoted to the monkeys, the third to the lemurs and bats, the fourth to the hedgehogs, moles, and cats, which latter continue into the fifth, and are succeeded by the genets, civets, and

Bears Walking.
(From Lydekker's "Royal Natural History.")

mongooses. In the sixth part are fossil hyænas, dogs, and foxes; in the seventh, bears, raccoons, and weasels; in the eighth, seals and their kind, and we are introduced to the oxen, which continue in part 9, and are succeeded by the sheep, goats, and antelopes. Part 10 carries us well into the deer. Mr. Lydekker's descriptions are distinctively and pleasantly written, and much interesting and anecdotal matter is incorporated. Special attention too has been paid to the occurrence of each form, when known, in the fossil state.

Taken as a whole, we do not know of any work that may be so satisfactorily recommended to those who wish an acquaintance with the mammalia. An additional interest will be found in the living animals at the Zoological Gardens if studied family by family by the aid of Mr. Lydekker's Natural History.

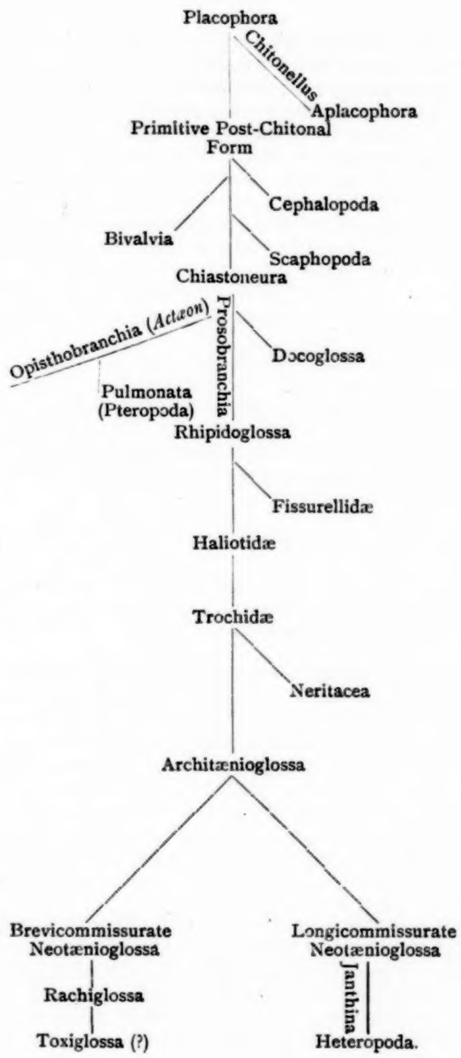
Limpets and their Allies.

Studien über Docoglosse und Rhipidoglosse Prosobranchier nebst bemerkungen über die phyletischen Beziehungender Mollusken untereinander. By Dr. B. Haller. 4to. Pp. iv., 173; 12 plates, coloured. Leipzig: Engelmann, 1894.

The greater part of this interesting and most important work is taken up with the description of the anatomy and morphology of the Docoglossa, or Limpets, and the Rhipidoglossa, or group to which the Keyhole-Limpets, the Ormers, and the Topshells belong. The Docoglossa are divided by the author into Bibranchia, comprising the two-gilled *Propilidium*, Monobranchia, or those with the single neck gill, and the Cyclobranchia, or those, like the common Limpet, which have a circlet of gills. Where the Lepetidæ, which have neither the neck-gill nor gill-circlet, come in is not apparent. Of these three groups, the first named is considered to be the oldest, the others following in their order. Of the Rhipidoglossa, the genus *Pleurotomaria* is held to be the oldest, though Dr. Haller speaks of its anatomy being quite unknown, having apparently entirely overlooked Dall's account of it in the Bulletin of the Museum of Comparative Zoology at Harvard College, xviii. (1889), pp. 397–403, a circumstance not altogether to be wondered at considering the way in which the eminent American naturalist buries his best results in papers whose titles give no clue to their valuable contents.

Dr. Haller's last twenty pages are, however, the most interesting from a general point of view, containing as they do his conclusions concerning the phylogenetic relations of the various members of the Molluscan stem. These conclusions are of great importance, and will be best made clear by reproducing his own diagram.

On contrasting this with the tree drawn up by Pelseneer (Natural Science, iii., p. 37), which is now very widely accepted, certain prominent differences, apart from mode of drawing up, become apparent. Thus, to begin with, Dr. Haller takes the Chitons as his base, and derives all the others from it, including the Cephalopoda, although both their embryology and geological history are against such a supposition. Other equally startling conclusions, such as the classing of the Pteropoda with the Pulmonata, the sea-butterflies with land-snails, may be observed, and further comments can be safely left to the reader. Whether Dr. Haller's tree will come unscathed through the fire of criticism is doubtful, but every thoughtful attempt to unravel the genetic relationships of a group is of extreme value, and we are proportionately grateful to the author for this latest contribution to Molluscan literature.


Placophora
Chitonellus
Aplacophora
Primitive Post-Chitonal Form
Cephalopoda
Bivalvia
Scaphopoda
Chiastoneura
Opisthobranchia (Actæon)
Prosobranchia
Docoglossa
Pulmonata (Pteropoda)
Rhipidoglossa
Fissurellidæ
Haliotidæ
Trochidæ
Neritacea
Architænioglossa
Brevicommissurate Neotænioglossa
Longicommissurate Neotænioglossa
Rachiglossa
Janthina
Toxiglossa (?)
Heteropoda.

The Salmon of the Elbe.

Der Elbelachs: eine biologisch-anatomische Studie. By Professor Dr. Anton Fritsch. 8vo. Pp. 116, with 84 figs. and 1 coloured plate. Prague: F. Rivnac, 1894.

In the midst of numerous other occupations during the past twenty-five years, Dr. Anton Fritsch has been engaged in collecting information concerning the salmon and salmon-fisheries of the Elbe. So long ago as 1871 he published some preliminary observations on the subject, followed later by other notes; and now he has done good service by summarising the results of his long-continued research in the little profusely illustrated volume before us. The first part of the work is devoted to a sketch of the life and migrations of the salmon; the second and third parts deal respectively with economics and the anatomy of the fish; and a few remarks are appended in reference to the future of the salmon-fishery of the Elbe. The volume is issued with the aid of a grant from the Bohemian Parliament, and will prove of great value to those concerned in the industry of which it treats.

The account of the salmon fisheries, as might be expected, occupies the greater part of the work; but the chapter on anatomy is by no means meagre, and the descriptive details are illustrated with numerous drawings by the author. There are also sketches of external features which vary at different periods, and a few illustrations of the principal parasites known to infest the fish.

The Organisms of Ponds and Rock Pools.

Ponds and Rock Pools: with Hints on Collecting for, and the Management of the Micro-aquarium. By Henry Scherren. Small 8vo. Pp. 208, with numerous woodcuts. London: Religious Tract Society, 1894. Price 2s. 6d.

This attractive little volume comprises a series of articles recently contributed by Mr. Scherren to the *Leisure Hour*. It is adapted as well for the general reader as for the beginner, and differs from most works of its kind in being much more than a second-hand compilation. The author is an enthusiast in his subject, and has himself verified most of the facts he records; while all the hints as to collecting and the management of the aquarium are the result of wide personal experience. It is, in short, a noteworthy book, not only to be recommended as a guide to the amateur, but one likely to infuse enthusiasm in a youth who has previously had little opportunity of looking into the subject of which it treats.

Mr. Scherren's hints on "pond and rock-pool hunting" are, as we have said, thoroughly practical. He writes not only for the wealthy pedant, who can afford elaborate apparatus, but remembers also the humble school-boy who is obliged to make his own simple tackle. The introductory chapter on these matters occupies over thirty pages, and has nine illustrations. The second chapter deals with "the beginnings of life," and emphasises more clearly than usual the difference between unicellular and multicelluar animals. The remarks on classification are also surprisingly up-to-date for a popular work. There are good figures of the *Amœba*, *Vorticella*, and other typical Protozoa. Chapter III. treats of sponges, hydrozoa, etc., and is illustrated by several original figures, chiefly of marine

animals. The common *Hydra*, as usual, merits a full share of attention, and we are pleased to observe some reference to the correction of the commonly-accepted errors resulting from Trembley's experiments on this animal. The worms follow, and are described in the words of Huxley as a "heterogeneous mob." Rotifers, of course, are treated at considerable length, and there are good figures—some original, some after Hudson. The Polyzoa are also very well described. Chapter V. relates to the larvæ and more minute forms of starfishes, arthropods, and molluscs; but this would bear much expansion and further illustration with advantage.

The concluding chapter on the micro-aquarium will be found as useful as that on collecting, and interest is added to it by a reproduction of the original engraving of Trembley's study, showing the glass jars in which the well-known experiments on *Hydra* were made. A view of the author's arrangements forms the frontispiece. A good index is appended.

A History of the World.

Histoire du Monde: son Evolution et sa Civilisation. By Etienne Guyard. 8vo. Pp. 700, with Schrader's Planisphere. Paris: published by Author, 5, Impasse Nicole, 1894. Price 7 fr.

This is a somewhat strange work by a former professor in the Imperial Military Academy of Japan. It is the first volume of a projected series of five, intended to be a popular account of the astronomical and geological history of the globe itself, an outline of the evolution of its animals and plants, and a general synopsis of the history of man from primæval times to the conditions of modern political and social life. The volume before us treats of astronomy, botany, zoology, geology, anthropology, and the history of the progress of organic evolution, and bears evidence of having been most laboriously compiled from the best modern sources. So far as we have been able to test them, the statements are generally clear and reliable; while to those who desire only a superficial knowledge of the multifarious subjects treated, and do not object to a somewhat bald, encyclopædic style, the work will doubtless prove itself very useful. The great inconvenience in the book, in our opinion, is, that the author has adopted the Oriental custom of placing the title page at what we term the end, while the pagination is exactly the reverse of the ordinary. Moreover, there is a great dearth of illustrations. The appended maps, however, are very good, and the bulk of the volume would doubtless be too much increased if every section were illustrated in the manner customary in special treatises.

Plant Physiology.

Experimental Plant Physiology. By Dr. Walter Oels. Translated and edited by D. T. Macdougal, University of Minnesota. 8vo. Pp. 86, with 77 Illustrations. Minnesota: Morris & Wilson, 1894.

This English edition of Dr. Oel's admirable little book will prove a useful guide to a series of elementary experiments in illustration of the most important principles of plant-life. The many clear and excellent illustrations form a valuable supplement to the descriptions. The contents are arranged under the following headings: derivation of nutriment from soil and water; transpiration; photosynthesis (a term suggested by Professor Conway Macmillan to express

the decomposition of carbon dioxide and water by chlorophyll in the presence of sunlight); respiration and metabolism; geotropism; heliotropism; warmth; growth; movement; relation of plants to animals. The last-named is treated in a very perfunctory manner, only three simple experiments being suggested, whereas it is a subject which offers an almost infinite scope, and is, moreover, of the highest interest. The book is nicely printed, though misprints are not rare, and sentences are occasionally a little involved. We think, too, it is unnecessary to speak of sodium chloride as cooking-salt in connection with plant nutrition. The apparatus required for the various experiments is fairly simple and will be found in any moderately well-equipped high school or college laboratory. As is suggested in the introduction, good plant material is an important part of the equipment, and it is therefore essential that every laboratory undertaking the work should have ready access to a greenhouse or small botanical garden.

The Carboniferous Insects of Commentry (Allier), France.

The Coal-measures of Commentry, Central France, have in recent years, under the skilful exploration of their director and engineer, M. Henry Fayol, yielded a valuable collection of fossils illustrating the fauna and flora of the Upper Carboniferous period.

The specimens were placed in the hands of specialists; the plant remains were delivered over to MM. Renault and Zeiller, who have already published the results of their examinations, so likewise the fishes, by Dr. Emile Sauvage, and the insects were entrusted to M. Chas. Brongniart, of the Natural History Museum, Paris.

M. Brongniart has now completed his share of the work, and has embodied his results in a monograph embracing Neuroptera, Orthoptera, and Homoptera.

The work forms a volume of about 450 pages in 4to, and an atlas of 37 plates in folio.

Sixty new genera and 100 new species are described.

The work is accompanied by a detailed examination of the nervation of the living Neuroptera, Orthoptera, and Fulgoridæ.

The value of this publication, and its interest to palæontologists and entomologists tracing out ancestral forms cannot be overstated, as it gives an insight into an insect fauna rich beyond compare, and proves that those "forests primæval" were neither tenantless nor voiceless.

Our knowledge of palæozoic insects has been hitherto scanty, owing to the rarity of specimens and the fragmentary condition, for the most part, in which their fossilised remains have been found. This has all been changed by the remarkable deposit at Commentry, which has yielded ten times more specimens than all other localities in Europe and America conjoined. Not only are the specimens more abundant, but they are found in a state of entirety and preservation most advantageous for study—a fact no doubt due to the fine matrix in which they are embedded and the conditions under which they were entombed.

Mr. S. H. Scudder, the eminent American entomologist (in *Amer. Journ. Sci.*, Feb., 1894), writing on these insects and M. Brongniart's drawings, which he has recently had the opportunity of examining in Paris (facts which I can confirm from my own inspection of them two or three years ago), says: "I have had the opportunity, through

the kindness of M. Brongniart, of seeing not only a considerable part of this collection, but also the illustrations prepared by M. Brongniart himself from the choicest specimens; illustrations made with a care and exactitude which leave nothing to be desired, and which are now nearly completed after a labour of ten years, so that we may hope soon to be favoured with his final work. Leaving the cockroaches out of account, to which M. Brongniart will give his attention later, the number of these illustrations, their variety, the extraordinary character of the insects themselves, and their rare perfection, leave not the least room for doubt that when his work appears, our knowledge of palæozoic insects will have been increased three- or fourfold at a single stroke, and an entirely new point of departure for the future opened. No former contribution in this field can in any way compare with it, nor even all former contributions taken together. Besides, it will offer such a striking series of strange forms as cannot fail to awaken the attention of the least incurious. One may not enter into details, but mention may simply be made of one species, regarded by M. Brongniart as one of the forerunners of the dragon flies, in which the wings have an expanse of considerably more than two feet (or about 70 centimetres), and of which several specimens are preserved. It is a veritable giant among insects."

MARK STIRRUP.

L'ANTHROPOLOGIE. Paris: vol. v., nos. 1, 2, 3. 1894.

THERE are several papers of geological interest in these three parts of "L'Anthropologie." Emil Cartailhac describes the flint implements which have been secured from L'Herm Cave, Ariège, and Marcellin Boule has a note on the lower jaw of *Gulo luscus*, and in a similar bone of an enormous *Felis* from the same cavern. This second jaw, which measures 295 mm., is larger than any yet recorded, and M. Boule names it *F. leo*, race *spelæa*. Ed. Piette writes some notes illustrative of primitive art of the horse and reindeer periods. Good illustrations accompany the paper, one of especial interest showing an auroch's head cut into a piece of stone. Another article by Salomon Reinach treats of European primitive sculpture showing Græco-Roman influence, but this, though of extreme interest, deals more or less with historic peoples.

IN a recent paper entitled "The Mammalia of the Deep River Beds" (*Trans. Amer. Phil. Soc.*, vol. xvii.), Professor Scott makes some valuable additions to our knowledge of the American Equidæ, and describes some forms of which only brief notices had previously been given. Of these, Desmathippus is notable since it fills almost the only important gap left in the phylogenetic history of the horse, coming midway between Miohippus and Protohippus. The teeth are short-crowned with the valleys partly filled by cement; the feet possess fairly well-developed lateral digits. Another interesting form is a true Anchitherium, *A. equinum*, nearly allied to the European *A. aurelianense*, but in some respects more horse-like. The author shows that these are good reasons for regarding Anchitherium as a side branch of the equine phylum which has left no descendants, and considers that the genus must have arisen in America, probably from Miohippus. *A. equinum* occurs in the upper division of Deep River Beds which is taken by the author as approximately equivalent in age

to the Miocene of Sansan, a somewhat lower horizon than that to which it has previously been referred.

The paper also includes descriptions of the Carnivora, Rodentia, and Artiodactyla from the same deposits.

We have received a "Geological Sketch Map of Western Australia, 1894, by Harry Page Woodward, Government Geologist, Perth." The Recent and Tertiary rocks form a fringe of varying width along nearly the whole of the coast, and are somewhat extensively developed in the southern portion, or Eúcla division. The Mesozoic rocks, bounded on the west by the Recent and Tertiary, and on the east by the Metamorphic and Palæozoic rocks, extend from near Gingin, north of Perth, northwards to Cape North-West. The Palæozoic rocks (Carboniferous and Devonian) are largely developed; they have an enormous extent in the neighbourhood of the Gascoyne, Henry, Ashburton, and Fortescue rivers, and also in the Kimberley division; they appear also on the Irwin river and to the north of Albany. The Metamorphic and Igneous rocks are shown to occupy a much greater area than even the Palæozoic; they stretch through the south-west and Gascoyne divisions as a broad band, nearly parallel to the western coast, and occur also in the north-west and Kimberley divisions. The various gold-fields are indicated, as well as the districts in which copper, lead, tin, and coal have respectively been found. This map should prove of the greatest value to all who are interested in the geology of Western Australia.

We may add that three other works on Western Australia are also reviewed in the September number of the *Geological Magazine*. It is rapidly taking a foremost place among the Australian colonies, and in extent of territory it greatly exceeds the others. A sum of £71,482 has lately been voted for public works and buildings, including £2,000 for the building of a museum in Perth.

In the first part of the new *Proceedings of the Victoria Institute of Trinidad*, Mr. R. J. L. Guppy gives an account of the edible molluscs of Trinidad. Two or three kinds of oysters are eaten by Europeans, but among the lower orders they are in little favour. *Asaphis deflorata*, when well cooked, Mr. Guppy pronounces most delicious; *Mytilus brasiliensis* and *Venus granulata* are common in the markets. The North American clam is a *Venus*, but is a very different species, and is absent in Trinidad. *Donax*, and that large bivalve *Pinna*, are commonly used, but Mr. Guppy has no personal experience of them. Nearly all the fresh-water molluscs are eaten. The land molluscs are less in request, although the large *Bulimus oblongus* is said by French writers to have flesh which, though somewhat leathery, possesses a delightful aromatic flavour. What Mr. Guppy believes to be the kitchen middens of the aboriginal inhabitants show that a larger number of molluscs were consumed by them.

Mr. R. J. Lechmere Guppy sends us a reprint of his paper on the Microzoa of the Tertiary and other rocks of Trinidad and the West Indies. The paper appeared in the *Journal of the Trinidad Field Naturalists' Club*. It sums up the results of former publications by the

same author, and adds many new observations. It is accompanied by a table showing the distribution of foraminifera in the "Cretaceo-Tertiary" rocks of Trinidad.

We congratulate the botanical seminar of Nebraska University on the work achieved in connection with the flora of the State during last year, as indicated in the Report for 1893. The additions number 182, and bring the total number of species reported up to 2,820. Mr. Clements describes several new species of Fungi, and new hosts are recorded for fifteen others. It would be well if some separation of the great groups were indicated; as it stands, Algæ, Fungi, Mosses, and flowering plants run on in one unbroken series. But the great fault lies in the unsparing use of a trinomial nomenclature; if a plant is a variety let it be so stated—the practice of stringing names together only leads us back to pre-Linnean times. The pamphlet also contains a revision of the nomenclature of the Nebraska Polypetalæ, according to the latest ideas; *Bursa bursa-pastoris* does, however, call forth a protest from the author, Mr. P. A. Rydberg. Mr. F. E. Clements supplies a list of botanical expeditions in Nebraska from 1803 up to date, and Mr. R. Pound a bibliography of the flora of the State.

The Orchid Review (August) suggests that in the hybrid Disas, of which we have seen and heard so much lately, we may get a race of easily cultivated common greenhouse orchids. They grow like weeds, only requiring suitable compost, plenty of water, a little shade, and protection from frost. They will certainly be a beautiful addition to the list of plants within the reach of the amateur. The great centre for Disas is at the Cape, whence they run up into tropical Africa, occurring as far north as the mountains of Abyssinia, and also in Madagascar.

Die Natürlichen Pflanzenfamilien, under Professor Engler's direction, progresses slowly. The last issue, a double one, containing nos. 106 and 107, completes section 6A of part iii., and includes the remainder of Cactaceæ, by K. Schumann, and Geissolomaceæ, Penæaceæ, Oliniaceæ, Thymelæaceæ, and Alæagnaceæ, by E. Gilg. Also a portion of Borraginaceæ, by M. Gürke, belonging to part iv.

A *résumé* of the Geology of Spitzbergen is being published in the *Feuille des jeunes Naturalistes*, by M. Gustave Dollfus. The writer gives a geological map after Nathorst, and reviews the previous writings on the geology of the island. The Tertiary, Jurassic, Triassic, Permo-Carboniferous, Devonian, Silurian (?), and Archæan rocks are represented, and various volcanic rocks have been recorded.

In the *Feuille des jeunes Naturalistes* for September, M. G. de Rocquigny has an interesting note on the pairing of butterflies of different genera. The forms noted were a male of *Satyrus janira*, and a female of *Vanessa urtica*. During the flight *S. janira* was carried by *V. urtica* and hung lifeless (*inerte*). The observation was made at Allier, on June 23, 9.45 a.m.

We have received from R. Friedlaender & Sohn, of Carlstrasse, Berlin, nos. 9 to 13 of their fortnightly publication. It professes to give a list of all new books and of the numbers of periodicals as they are issued which deal with Natural Science. It seems to us a publication to be commended to all scientific persons, and may be had post free for the modest annual subscription of four shillings.

Yet one more local scientific publication. *Madagascaria* is a monthly, dealing with the zoology, geology, botany, and anthropology of Madagascar, which Mr. F. Sikora intends to produce, starting on October 1, at Antananarivo, a place that as yet has not got beyond its well-known *Annual*. Yearly subscriptions of six shillings and fourpence (eight francs) may be sent to the Comptoir National d'Escompte in Paris, addressed, "Pour Mr. Sikora à Antananarivo."

OBITUARY.

FRANÇOIS CLÉMENT MAILLOT.

BORN 1804. DIED 1894.

SO commonplace is now the employment of quinine in the treatment of malarial fevers that the very names of those who first advocated its use are already omitted from our text-books, and in danger of passing from our memories.

The death of Dr. Maillot, at the advanced age of 91, recalls the services he rendered sixty years ago, not only to medicine, but to the world at large. Educated at Metz, he entered the career of military medicine, and in 1834 he was stationed at Bona, in Algeria, then newly colonised by France. The malarious climate was producing an enormous mortality among the new-comers, and it seemed an open question whether it would be possible to retain the colony. Maillot, who had already had some experience of malaria in Corsica, at once devoted himself to the study of the intermittent fevers of the district and their treatment by large doses of quinine, and with such success that the mortality from malaria is said to have fallen from 25 to 5 per cent. The actual discovery of the value of the cinchona alkaloids in the treatment of ague was, of course, long before this; but it may at least be claimed for Maillot that his advocacy of the employment of quinine not only popularised the remedy and saved thousands of lives, but demonstrated also the possibility of European colonisation of a highly malarious country.

A tardy recognition of his services to military medicine was made in 1888, when a pension of 6,000 fr. was conferred upon him; but it was not till last year that his numerous writings, dating from 1834 to 1887, and all devoted to this one subject, were collected and published in connected form.

REV. W. M. HIND, LL.D.

BORN 1815. DIED 1894.

WE regret to announce the death of the Rev. W. M. Hind, LL.D., Rector of Honington, Suffolk, at the ripe age of 79. For a long period he had devoted the greater part of his spare time to botanical field work, and was possessed of a wide and accurate knowledge of our British wild plants, many of the rarer forms of which he had cultivated and observed. Born in Belfast of English

parents, and educated at the Academy, he passed to Trinity College, Dublin, where he graduated B.A. 1839, M.A., 1842. In 1876 he edited an enlarged and revised edition of J. Cosmo Melvill's "Flora of Harrow," towards the original edition of which he had very largely contributed when a curate at Harrow. In 1875 he accepted the Chancellor's living of Honington, Suffolk, and immediately set to work to compile a flora of Suffolk, which he published in 1889. Having collected a large Herbarium of Suffolk plants, they were presented to the Ipswich and East Suffolk Museum. In 1870 he presented a very large and almost complete collection of British plants to the Trinity College, Dublin, Museum, and as an acknowledgment of the gift, was presented with the honorary degrees of LL.B., LL.D. by a special grace of the Senate. Dr. Hind was a man of high intellectual attainments and wide and varied reading. Essentially of a scientific mind, and an ardent and enthusiastic worker in his own subject, his wide botanical knowledge was always at the service of others.

The death is announced of Admiral Sir Edward Augustus Inglefield, K.C.B., who commanded three Arctic Expeditions, the first in 1852 in search of Sir John Franklin, and added much to our knowledge of the geography of the Arctic Regions. He had reached the advanced age of 74 years.

We have also to record the deaths of Pierre Louis Jouy, who had studied the ornithology of Japan, Korea, S. Arizona and Mexico; Dr. Erich Haase, Director of the Royal Siamese Museum in Bangkok, and a special student of the Arthropoda; Dr. Adolph Hannover, of Copenhagen, a well-known Anatomist and Histologist; Professor Michele Lessona, President of the Academy of Sciences and Director of the Zoological Museum in Turin; the nonagenarian, Dr. Louis de Coulon, one of the founders and, for most of its existence, the President of the Society of the Natural Sciences in Neufchatel.

The death of Professor Hermann von Helmholtz, on September 9, deprives Physiological Science of one of its most brilliant investigators. We hope to give some account of his work next month.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

Dr. J. Playfair McMurrich has resigned his position in the University of Cincinnati, to accept the Professorship of Anatomy in the University of Michigan, at Ann Arbor.

Several botanical appointments have recently been made. Mr. T. H. Kearney, jun., succeeds the late Dr. Moring as Curator of the Herbarium of Columbia College, New York. Dr. W. Scott has been appointed Director of Forests and of the Botanical Garden in Mauritius. Dr. A. Zimmermann becomes Extraordinary Professor of Botany in the University of Tübingen; and Dr. Solereder is now a Curator at the Munich Botanical Institute.

We regret to learn of the retirement of Dr. E. P. Ramsay from the Directorship of the Australian Museum, Sydney. Dr. Ramsay has been in failing heath for some time, and the duties of his office have fallen upon Mr. Robert Etheridge, jun., who has now been chosen as successor. Mr. Etheridge entered the Australian Museum as Palæontologist in 1887 on leaving the British Museum, and will assume his new office on January 1, 1895.

At the Geological Congress at Zurich it was decided, upon the instigation of Captain Marshall Hall, supported by Professor F. A. Forel, to appoint an International Committee for furtherance and record of observations upon existing glaciers in different parts of the world. We will give further information so soon as the committee is constituted.

The Superintendent of the Indian Museum, Calcutta (Surgeon-Captain A. Alcock), has issued an admirable "Guide to the Zoological Collections exhibited in the Invertebrate Gallery of the Indian Museum." It is a little volume of 155 pages, and forms a most concise introduction to Invertebrate Zoology.

The Bristol Museum is closed for internal and external decoration, and is now being put into a thorough state of repair. Wires have been introduced for electric lighting, the dilapidated cases have been repaired, and the long-neglected collections will thus soon be again safely housed. The advent of municipal control has effected all these improvements, and it would be well if a few more of our languishing provincial museums could be similarly transferred to their respective towns. Mr. Edward Wilson, the energetic Curator, has just issued a new edition of his popular penny Guide, which continues to have a large sale.

M. Stanislas Meunier has inaugurated an annual exhibition of geological specimens at the Museum of the Jardin des Plantes, Paris, for the purpose of assisting geologists to follow and verify the work of the year. The exhibition is to be

not merely national, but to contain contributions from the various foreign Geological Surveys. To illustrate the past year the Geological Survey of Great Britain has lent specimens of the Archæan and pre-Cambrian rocks of North-west Scotland, geological maps in course of preparation, and a collection of new geological works and photographs. Signor Pellati, the Director of the Geological Commission at Rome, has sent specimens illustrative of recent Italian researches. The United States and Queensland Surveys have sent copies of their publications; and the Imperial University of St. Petersburg has lent a variety of specimens of platiniferous rocks, the discovery of which is of great importance. A small guide-book of 24 pages is issued in connection with the exhibition.

Important changes are taking place in the Palæontological Department of the U.S. National Museum at Washington. The Honorary Curator of this Department is Mr. Walcott, the new head of the Geological Survey. Till last July there were five honorary curators, who, though they advanced the collections, had little time to spare for purely curatorial work. Now, however, Mr. Charles Schuchert has been appointed Assistant Curator, with a salary, and is able to devote his time to practical improvements which will doubtless bring the exhibition series of this Department to the high level attained in the rest of the Museum. For the present, exhibition will be limited to a good representation of the North American fossil faunas geologically arranged, but not as yet, owing to want of space, divided into geographical provinces. The invertebrates will be displayed in the table-cases, the plants and vertebrates in the wall-cases. As soon as possible Mr. Schuchert will begin a synoptic collection illustrative of all fossil genera, in which each class will begin with a series explaining the terminology, after the fashion of the Crinoids in the Geological Department of the British Museum.

In addition to much information that has already appeared in our pages, the *Annual Report of the Delegates of the* [Oxford] *University Museum* for 1893 contains the following items of interest. In the Ethnographical Department of the Pitt-Rivers Museum series have been arranged which illustrate Magic, the early development of Writing, Lighting, Smoking, Treatment of the Dead, and Primitive Pottery. As evidence of original research conducted in his department, the Professor of Physiology quotes a list of six papers by five authors. Similarly, the Professor of Comparative Anatomy enumerates three ladies and six gentlemen who have used his senior laboratory for original investigations. The Professor of Zoology states that Dr. F. A. Dixey has studied the Pieridæ in the Hope Collections, in the preparation of an important memoir upon the evolution of the wing-markings in this group of Lepidoptera; the specimens have at the same time been arranged in a single series. Colonel Swinhoe has continued his Catalogue of Oriental Heterocera in the Hope Collections, and the second volume, containing the Pyrales and the Noctuæ, will soon be issued. The Hope Professor is breaking the tenth commandment in respect to the Mathematical Professors, whose room is better than their company. The delegates concur. The fittings of the new geological laboratory are all but complete, and advanced teaching can now be carried on in comfort, and with greater thoroughness than heretofore. A rock collection for teaching purposes is being systematically arranged and illustrated by explanatory labels. We fully agree with the Professor of Geology when he says, "Re-arrangement of the fossils is urgently needed, and it is hoped that it may be begun shortly. It will be a long and heavy piece of work, and skilled assistance will be absolutely necessary to carry it out successfully." The Professor of Mineralogy is taking in hand the re-arrangement of the mineral collection; this, too, not before it was wanted.

By the provisions of the will of the late Dr. William Johnson Walker, two prizes are annually offered by the Boston Society of Natural History for the best

memoirs written in the English language on subjects proposed by a committee appointed by the Council.

For the best memoir presented a prize of sixty dollars may be awarded; if, however, the memoir be one of marked merit, the amount may be increased to one hundred dollars at the discretion of the committee. For the next best memoir, a prize not exceeding fifty dollars may be awarded. Prizes will not be awarded unless the memoirs presented are of adequate merit. The competition for these prizes is not restricted, but open to all.

Each memoir must be accompanied by a sealed envelope enclosing the author's name and superscribed with a motto corresponding to one borne by the manuscript, and must be in the hands of the Secretary on or before April 1 of the year for which the prize is offered.

The Subjects for 1895 are:—

(1) A study of the "Fall line" in New Jersey.

(2) A study of the Devonian formation of the Ohio basin.

(3) Relations of the order Plantaginaceæ.

(4) Experimental investigations in morphology or embryology.

The Subjects for 1896 are:

(1) A study of the area of schistose or foliated rocks in the eastern United States.

(2) A study of the development of river valleys in some considerable area or folded or faulted Appalachian structure in Pennsylvania, Virginia, or Tennessee.

(3) An experimental study of the effects of close-fertilisation in the case of some plant of short cycle.

(4) Contributions to our knowledge of the general morphology or the general physiology of any animal except man.

In all cases the memoirs are to be based on a considerable body of original work, as well as on a general review of the literature of the subject.

CORRESPONDENCE.

Plateau Man at the British Association.

In reading over the report of the British Association discussion on Professor Rupert Jones's paper in the last number of Natural Science,[1] I am struck with a series of curious coincidences, and as I consider they are calculated to propagate a very wrong impression, I venture to ask to be allowed to point them out. I do not for a moment suppose it was intentionally done, especially in the face of the opening sentences, still it is very strange that almost every adverse criticism raised by the speakers is given, while nearly all arguments for, and admission of, the antiquity of plateau man are overlooked, and a summing-up given which may not always be taken in the way which a further examination of the facts of the case would demand. This is all the more to be regretted, as every supposed argument urged against plateau man rises on the speakers not being in possession of all the facts of the case. If it were simply an instance of various interpretations being put upon well-known phenomena of the origin of which we know but little, differences of opinion are all that could be expected; but in this subject—as indeed is elsewhere suggested in reference to all others—the value of one's opinion is not to be estimated alone by qualifications in other branches of science, however nearly related they may be to the question at issue, but by the amount of careful, extensive, and prolonged research in and out of the field he has given to it, qualified by his ability to observe. I am very much tempted to answer the criticisms raised at the British Association meetings one by one, but for my confidence in the effect of the publication of the results of recent labours. Those who are in possession of the facts of the subject have but one opinion; those who differ from us will alter theirs as knowledge accumulates. Truth is doubtless as precious to them as to ourselves, although some people are apt at times to forget that change of opinion is an honest man's characteristic. I thus hasten to a further examination of the discussion. Now I think the way this can best be done is to go through the remarks of each speaker, in so far as they bear upon the point at issue. The first speaker was Mr. Whitaker, in whose speech I find the following:—"This certainly carries man back, locally at all events, beyond the time of the river gravels, which occur in the bottoms and along the slopes of the valleys." He appears (*Times* report) to have been followed by Mr. Montgomerie Bell, who said that, in his opinion, "they (the plateau relics) belonged to pre-Glacial or Pliocene times." The next speaker was Sir John Evans, who admitted "that the principal outcome of recent discoveries was, to his mind, the fact that the existence of Palæolithic man could be carried further back in time than the valley gravels, inasmuch as flints are found in gravels on the plateaux at far higher levels." Sir John was followed by Dr. Hicks—who has had far too much experience in this subject to deny the pre-Glacial existence of man in Britain,—and he pointed out the great concession that had been made by Sir John Evans, and also how the same had been wrung out of the veteran Professor Prestwich by the repeated discoveries of man's work in pre-Boulder Clay deposits. Then followed Professor Boyd Dawkins, who made some sweeping assertions, which, like the adverse criticisms of other speakers, can be flatly challenged; but so far as the question at issue is concerned, he said he agreed with what Mr. Whitaker and Sir John Evans had said, and made no exception to the extension of time they had granted. General Pitt

[1] Reprinted in full in the present number, pp. 269-275.—Ed.

Rivers, from another standpoint, argued the *necessity* of pre-Palæolithic man. Sir Henry Howorth gave it as his opinion that the only evidence available for testing the relative age of the plateaux gravels goes to show that they are older than the distribution of the so-called Glacial Drift. With reference to Mr. Clement Reid's remarks, I am pleased to say he is not informed up-to-date; there are numerous localities now in various parts of the kingdom which have produced these relics of greater antiquity. Among the latest I might mention the work of Dr. Blackmore in his own neighbourhood, and the yields of the hill-drifts in Essex; the Boulder Clay of Finchley, and of deposits older than these in Middlesex, Essex, Suffolk, and Norfolk. The theory suggested by the concluding speaker, variously reported as Lt.-Col. Godwin and Col. Godwin Austin, calls for the existence of huge glaciers in the Weald, which would certainly take us back very much further in time than many anthropologists have been in the habit of admitting for man.

I cannot help thinking that a perusal of these facts will better emphasise your opening remarks and give a truer idea of the state of scientific opinion of to-day upon this point, and show that the verdict of the discussion is that opinions are divided, but that the affirmatives are in excess of the negatives; this in proportion to the greater the knowledge of the subject possessed by the respective speakers. It must also be admitted that it was quite lamentable to observe upon how little practical knowledge of the deposits the adverse criticisms were based; one speaker, at least, still confounding them with "the Clay-with-flints."

W. J. Lewis Abbott.

The Museums' Association.

The note headed "Science at a Picnic," in your issue of August, is so entirely misleading, so far as it regards the Museums' Association, that we ask permission to offer some corrections, especially as the note has evidently been written by some one not present throughout the meetings. The meeting commenced on Tuesday, June 26, with visits to various scientific institutions, lasting four hours, and finishing with the President's address at night, the whole time occupied being five and a half hours. On Wednesday three and a half hours were devoted to papers and discussions, and another three hours to the inspection and discussion of the arrangements and contents of various museums. On Friday another three and a half hours were given to papers and discussions, followed by visits to museums afterwards. And yet the writer of the note speaks of five hours devoted to business, and talks of papers being burked! All the papers in the programme were submitted to the meeting, except one by a member who, although invited two months beforehand to give notice of his paper, sent no intimation of it till the programme was printed, and then appeared with some notes which he wished to descant upon, and was allowed to do so as long as time permitted. Exception may possibly be taken to a day's excursion finding a place in the programme of the meeting; but as curators have few facilities of exchanging ideas one with the other, generally to their mutual advantage, the value of such an arrangement will, we think, be quite evident to most people, and we believe that "shop" was the predominant subject of conversation, even on that day. The Local Committee in Dublin, in fact, showed the truest spirit of hospitality in affording to the Association every opportunity of prosecuting its work in the direction of acquiring knowledge of the excellent museum work that is carried on in that city, and if the writer of the note thinks that no good can accrue from such meetings, except what is derived from the reading and discussion of papers, he will find few people to concur with him in such a narrow and baneful view. It may interest him to know that the volume of proceedings of the Dublin meeting will be the largest that has ever been published by the Association, and the Local Committee are finding the funds for providing illustrations to some of the papers that were read.

H. M. Platnauer,
E. Howarth,
Secretaries Museums' Association.

Public Museum, Sheffield.

[We have much pleasure in printing this letter from the energetic secretaries of the Museums' Association. This body does, as we have often pointed out, such very valuable work, that we should greatly regret to injure it by any misstatements. We do not, however, gather that any of our statements are seriously traversed. We stated, correctly, that the time allotted to papers was five hours; we are now informed that seven hours were actually conceded, but this apparently includes the time spent on the formal business of the Association, which we did not reckon. We stated that "considerable complaint was heard at the way in which both papers and discussion were burked": we are now informed that all the papers in the programme were submitted to the meeting; this is quite true, but how many were cut short? how often was really useful discussion checked? and were the complaints we heard from numerous people present throughout the meeting entirely imaginary? We stated that "the insertion of a day's excursion between the two days of meeting proved vexatious to those who could ill spare their time"; we are informed that those who had time to spare talked "shop"; but no attempt is made to contest our statement. We fully agree that inspection of museums is and was a valuable part of the week's work; but waterfalls are not museums. Our only object was to utter a timely warning, so that the large amount of active and useful work hitherto accomplished by this Association at each of its annual gatherings might not be diminished by any misguided generosity.—Ed.]

Dr. J. M. Clarke writes to us with reference to the review of Hall and Clarke's "Handbook of the Brachiopoda," which appeared in our August number (p. 140), saying that the edition which fell into the hands of our reviewer "is purely incidental and accessory to the regular one. The work is published as pages 134–300 of the Eleventh Annual Report of the State Geologist, for the year 1891 (dated 1892, published 1894), and contains 22 lithographic plates." These plates were not seen by our reviewer.

We have received the following note from Professor F. A. Lucas, of Washington:—I was glad to see a note on Mr. Lydekker's new cetacean in Natural Science (August, p. 85). I have not the least doubt, from my acquaintance with whales, that what Mr. Lydekker saw was a humpback (*Megaptera*) lying at the surface and thrashing his fins about, which is a very characteristic trick of the genus.

TO CORRESPONDENTS.

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